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Croonian Lectures

ON

THE NUMERICAL METHOD, AND ITS APPLICATION TO THE SCIENCE AND ART OF MEDICINE.*

DELIVERED AT THE ROYAL COLLEGE OF PHYSICIANS, 1860.

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PART I.

Introductory Remarks. Explanation of Terms. The "Numerical Method" and the "Statistical Method". Meaning of the Terms "Statist" and "Statistics". Plan of the Course. Review of Sciences and Scientific Arts. The Universal Sciences. The Art of Calculation and the Science of Arithmetic; the Pure and Mixed Mathematics; Geometry. "Instruments of Calculation", as applied to the "Relations" and to the "Properties" of Matter. Names of "Recognition" and Names of "Identification". Sources of Uncertainty and Embarrassment in the Sciences which deal with the Properties of Matter. The Sciences and Arts of Construction: the Science and Art of Chemistry: the Science of Botany and the Arts of Horticulture and Farming: the Science of States and the Art of Government: the Science of Medicine and the Art of Healing.

MR. PRESIDENT AND GENTLEMEN,—When the President of this College informed me that he had done me the honour to appoint me Croonian Lecturer for the year, I first made it my business to inquire whether any, and, if so, what restrictions or limitations had been put to the choice of a subject. Finding that there were no such limitations or restrictions, I proceeded to inquire whether there was any peculiarity in the tastes or pursuits of the founder which would serve to indicate the sort of subject which, were he living, he would approve. I found, as the result of this inquiry, that, in addition to this endowment, Dr. Croone had founded no fewer than seventeen algebraic lectureships in the colleges of Cambridge. To one so anxious to encourage the culture of this universal arithmetic, a course of lectures having for its object to promote the application of arithmetic, in a less abstract shape, to medicine, would certainly have commended itself; and if any fresh reason for being satisfied with the choice which I had made were needed, I had before me the precedent of Dr. Bisset Hawkins, who more than thirty years ago made Vital Statistics the subject of his Gulstonian lectures, and those lectures the foundation of his well-known work on that subject.

Having, as I trust, justified my choice of a subject, I will only add to this short preface the expression of a hope that those who may have come here in the expectation of adding something to their stores of practical knowledge will bear with me if I seem to them to give to my subject, and especially to some parts of it, too abstract a character. As, in treating such a subject, it is impossible to be too exact and methodical, I will begin by defining the terms used in framing the title of the lectures.

My subject is, "*The Numerical Method, and its Application to the Science and Art of Medicine*". It will be seen that I have made deliberate choice of the term *numerical method* where some would have used the term *statistical method*. I have used the one form of words, and set aside the other, because I know nothing of a statistical method. I know of no use of numbers which claims to be peculiar to the Science of States; and I think that, whatever may have been the meaning of the barbarous word *statistics*, when first used in Germany, we ought now to limit it to what it always means in practice—a state-science built up by the aid of facts brought together in large numbers, and their results expressed in the terse, simple, and intelligible language of figures.

Several of our old writers of repute use the word *statist* in the sense of a scientific or speculative statesman; and statistical societies and congresses do practically make use of the term statistics to designate the science which our statists cultivate.

On the other hand, the term Numerical Method commends itself by obvious considerations, but especially by the fact that there is no science or scientific art in which a certain well defined use of figures, limited by rules, and governed by a logic of its own, does not find some place.

This, then, is the term which I shall use in these lectures, in preference to the more restricted and less correct phrase, *statistical method*.

I shall also use the terms *science of medicine* and *art of healing* in their largest and most comprehensive sense, so as to include the science and art of surgery and the science and art of hygiene, drawing my illustrations indifferently from the one or the other, as I shall find most convenient.

And now a few words on the object and plan of this short course of lectures. My object is to place the numerical method before you in the broadest and clearest light in which I am able to set it; and my plan, which is in accordance with this object, is to begin by passing in brief review some of the principal sciences and scientific arts, and endeavouring to find out, in respect of each in turn, what is the secret of the degree of excellence to which it has attained, and how far that excellence is to be traced to the facilities which it offers for the application of figures. This review, with that more special examination of our own science and art which may naturally be expected at my hands, will occupy the remainder of this lecture. The second lecture, with perhaps some part of the third, I propose to devote to the principles and rules of the numerical method, or, in other words, to the logic of large numbers. In what may remain of the course, I shall consider the extent to which this numerical method admits of application to the science of medicine, and in the art of healing; and, in doing so, I shall hope to remove some misapprehensions which seem to prevail in regard to this twofold application of the method, to the obvious discouragement of scientific inquiry, and the obvious injury both of the science and the art of medicine.

I now proceed, without further preface, to that brief review of sciences and scientific arts which, according to my plan, as just explained, is to form the staple of the remainder of this lecture.

Now, the first consideration which offers itself, as it were at the threshold of this inquiry, is, that there are certain universal arts, and sciences to correspond, which find an application, more or less extensive, in all the special and more limited arts and sciences. There is the *art of language*, spoken and written, and the *science of grammar*: the *art of observing*, and the *science of observation and experiment*: the *art of reasoning*, and the *science of logic*: and the *art of calculation*, with the *science of arithmetic*. It is with this art of calculation and science of arithmetic that we have now to do. This universal science attains to its highest development in algebra, and finds its most successful application in geometry and in the sciences known as the *pure mathematics*; but it also plays an important part in the *theory of probabilities*, and in that numerical method which may be said to be one of its principal branches, and certainly its most important one.

In reviewing the principal sciences and scientific arts, with the practical object of understanding the true position of the science of medicine and the art of healing, we perceive at once

* The three lectures, of which the course of Croonian Lectures consists, have been divided into portions of convenient length, and are here published very nearly as they were delivered. Some slight alterations are rendered necessary by the absence of the tables and curves referred to in the lectures.

that, if we place the arts and sciences in the order of the perfection to which they have attained, the first place must needs be assigned to the art of calculation and the science of arithmetic; for, in the first place, the words which we use to designate numbers, abstracted from all consideration of the properties of the things counted, cannot be used in two senses, nor can their meaning be misunderstood. The words *one, two, three, one hundred, one thousand, one million*, can have but one meaning: two and two must make four, simply because we have chosen to call two and two added together by that name. It matters not whether the things which are added to, or taken from, each other, are small or great—sands on the seashore, or worlds in the milky way—we use but one set of words, and we cannot use them amiss. In the second place, as arithmetic deals with numbers in the abstract—that is to say, without regard to the material properties of the things counted—all the fallacies arising from the imperfections of our senses, and of those faculties of the mind which take cognisance of the things of sense, are effectually excluded. In the third place, for words to which but one meaning can be attached, and for ideas so free from all the obscurities that belong to things of sense, it is possible to substitute symbols—figures for words, letters for known and unknown quantities, signs for simple phrases—so as to bring about a compactness of expression in the highest degree favourable to the carrying out of long and complicated processes of reasoning.

These, then, are the causes which combine to place calculation and arithmetic at the head of all the arts and sciences. But they merit that distinguished place for another reason already alluded to. They are of universal application in the service of all other arts and sciences; and the services rendered are of the most important kind—so important, that it is not affirming too much to say, that the extent and degree to which other arts and sciences admit of the application of figures, is the best test and measure of the position which they have attained.

The possession of a numerical theory—like the theory of gravitation in astronomy, or the theory of definite proportions in chemistry—confers, so to speak, a patent of nobility, and a title to precedence. The sciences which possess such theories are endowed not only with the gift of interpretation, but also with the gift of prophecy. They not only explain isolated facts and phenomena by a reference to general principles, but they make effectual guesses at the unknown, and foretell the very time and place of future events. It is as discoverers of numerical theories that the names of Newton and Dalton rank so high; it is the numerical exactness and precision of their prophecies which gives such a lustre to the names of Adams and Leverrier.

The *art and science of reasoning* might perhaps, with propriety, be classed with the art and science of arithmetic, as being equally certain, and equally extensive in its applications to other arts and sciences. Logic, in the limited sense of the science and art of reasoning from premisses assumed to be sound, owes its position among the arts and sciences to the successful analysis made by Aristotle of the mental process employed in every act of reasoning which the common sense of mankind recognises as valid. This analysis once made, it was easy to lay down abstract forms, expressed in that same terse symbolic language which I have just pointed out as so important an element in the science of calculation; and then to use these forms as tests by which every process of reasoning might be tried—as standards to which every sound process of reasoning might be made to conform.

Of the art of observing and the science of observation, which comprise the art and science of experiment (for experiments are but observations of phenomena prudently and skilfully prepared), I will only observe that this art and this science are also of universal application in the service of other arts and sciences; and that they have a logic of their own most happily set forth and explained in the lucid language of John Stuart Mill.

The art of verbal expression and the science of grammar have also their universal application in the service of science; but they do not suggest any considerations which need detain us.

I have spoken of an art of calculation and a science of arithmetic as being distinct from the mathematics of which they are generally held to form a part. Those who are familiar with mathematical studies will, I think, agree with me that such a distinction admits of being made, and that there is some advantage in insisting upon it.

The pure mathematics may be said to consist of algebra, or universal or abstract arithmetic, in which we reason, not by the aid of figures representing definite values, but by the aid of letters representing indefinite values; and of geometry, in which we trace the relations of straight and curved lines, on plain and curved surfaces, and of solid figures bounded by straight or curved lines, to each other; also, of the various so-called mathematical forms and figures, such as the angle, triangle, circle, square, and parallelogram.

All the truths of algebra and geometry may be, and have been, worked out by reasoning, without the aid of figures; and as these truths are pure deductions from *definitions* which could not be misunderstood, and from *axioms* which cannot be disputed or denied, they themselves cannot be gainsaid, and they accordingly constitute the only absolutely and necessarily certain portions of our knowledge. To these certainties—to these sure relations of lines and figures drawn on paper, or conceived in the mind itself—we may, if we please, apply the figures of arithmetic, and obtain results as certain as those relations themselves.

Then, again, there are other portions of the mathematics, technically known as the *mixed mathematics*, which apply the truths of geometry and the sure method of algebra to some well ascertained and well defined properties of matter. Hence the mixed mathematical sciences of *statics* (or the science of solids at rest), dynamics (or the science of bodies in motion), hydrostatics (or the science of fluids at rest), hydrodynamics (or the science of fluids in motion), astronomy, and optics. The leading truths of these sciences, too, may be worked out without the aid of arithmetic; though it is quite possible at every step to substitute the figures of arithemtic for the abstract expressions of algebra. As a general rule, however, we do not employ figures or numbers in the mixed mathematical sciences (or we use them only to a very limited extent) till we come to apply the truths of those sciences in actual practice.

I have spoken of geometry as a science based upon certain simple definitions and axioms, and having for its principal object the discovery of the properties of angles, triangles, and other simple figures. Now, these properties of angles, triangles, and other simple geometric figures, become, in the hands of scientific and practical men, so many *instruments of calculation*—instruments which admit of application as readily to lines measured out upon the earth's surface, as they did to lines drawn on paper. Hence the scientific art of land-measuring and surveying. But the use of these instruments of calculation is not confined to accessible objects, which we can measure by the foot-rule or the chain; they are equally applicable to those straight lines which represent the distances between the eye of an observer and the most remote and inaccessible objects. They may be applied to determine the height of a mountain which we cannot scale, or the width of a river which we cannot cross. And they are not even restricted to this service. They equally admit of application to visible objects altogether beyond our reach. They enable us to determine the relative positions and distances of the most remote of the heavenly bodies from each other, and from our own planet. Hence the *science of astronomy* and the *art of navigation*.

Of the perfection of this science, and the exactness of the corresponding art, Sir John Herschel shall give us an example. After remarking, with great truth, that to persons ignorant of physical astronomy, it must appear little short of a miracle "that a man, by merely measuring the moon's apparent distance from a star with a little portable instrument held in his hand and applied to his eye, even with so unstable a footing as the deck of a ship, shall say positively, within five miles, where he is on a boundless ocean," he gives a striking instance in point, on the authority of a distinguished naval officer, Captain Basil Hall. This officer "sailed from San Blas, on the west coast of Mexico; and, after a voyage of eight thousand miles, occupying eighty-nine days, arrived off Rio de Janeiro, having in this interval passed through the Pacific Ocean, rounded Cape Horn, and crossed the South Atlantic, without making any land, or even seeing a single sail, with the exception of an American whaler off Cape Horn. Arrived within a week's sail of Rio, he set seriously about determining, by lunar observations, the precise line of the ship's course, and its situation in it at a determinate moment; and, having ascertained this within from five to ten miles, ran the rest of the way by those more ready and compendious methods known to navigators, which can be safely employed for short trips between one known point and another, but which cannot be trusted in long voyages, where the moon is the only sure guide." The rest of the tale Sir John Herschel gives in the captain's own words.

"We steered towards Rio de Janeiro for some days after taking the lunars above described; and, having arrived within fifteen or twenty miles of the coast, I hove to at four in the morning till the day should break, and then bore up; for, though it was very hazy, we could see before us a couple of miles or so. About eight o'clock, it became so foggy that I did not like to stand in farther, and was just bringing the ship to the wind again, before sending the people to breakfast, when it suddenly cleared off, and I had the satisfaction of seeing the great sugar-loaf rock, which stands on one side of the harbour's mouth, so nearly right ahead that we had not to alter our course above a point in order to hit the entrance of Rio. This was the first land we had seen for three months, after crossing so many seas, and being set backwards and forwards by innumerable currents and foul winds."

In this anecdote, we have a remarkable example of a practical result brought about by the application of a few simple scientific principles, aided by calculations previously performed and ready for use. We have a ship conducted eight thousand miles over a pathless ocean by the aid of arithmetic and geometry, cooperating with the use of an instrument—"a little portable instrument"—measuring the moon's apparent distance from a star! Now, what is the secret of this surprising accuracy? Is it not this—that we are here dealing, not with the *properties of matter*, but simply with relative position; that we are applying the instruments of calculation of which I have been speaking, and the figures of arithmetic, not to the *properties*, but to the simplest *relations* of matter—to those "primary existences and relations" which are themselves abstract, and which, if there were but one form of matter in the universe, "we could not even conceive not to be"?

The science of astronomy and the art of navigation may, therefore, be safely placed at the head of the applied arts and sciences. They occupy the first place in point of accuracy, certainty, and precision; and this position of honour is obviously due to the causes to which I have referred. Astronomy has another claim to this foremost position, inasmuch as the universally received numerical theory of gravitation may be said to constitute one of its possessions.

If what I have just said of the cause of the high position occupied by the science of astronomy and the art of navigation be true, those sciences and arts ought also to occupy a high position which, like astronomy and navigation, have to do not so much with the *properties* as with the *relations* of matter; or if with properties of matter, then with such simple properties as can be submitted readily to experiment, and resolved, like distance and relative position, into an affair of lines and angles. *Optics* and *dynamics* evidently belong to this category; for the rays of light and the direction of forces may obviously be represented by lines and angles.

Without pretending to enumerate all the sciences and arts, or parts of sciences and arts, on which a character of accuracy and precision is conferred by the circumstance that they deal with the *relations* of material objects, and not with their *properties*, I shall content myself with observing, once for all, that wherever we can apply the figures of arithmetic and the instruments of calculation to the simple *relations* of matter, we can attain to a certainty little less complete than that with which we reason on the abstract truths of mathematics and geometry.

Leaving these highly favoured departments of human knowledge, I pass on, in the next place, to consider those arts and sciences which deal not with the *relations* but with the *properties* of matter. These properties are of two kinds—*mechanical* and *chemical*. The study of the mechanical properties of matter subserves all the varied purposes of manufacture, and gives rise, among other arts and sciences, to those of *construction*, which occupy the attention of the engineer and architect. The study of the more intimate composition of the particles or molecules of matter constitutes the science and art of *chemistry*, which finds its practical applications in almost every human pursuit.

The arts and sciences which have to do with the *properties* of matter are at once distinguished from those which treat solely of the *relations* of matter, inasmuch as they require, either as a constituent part of those arts and sciences themselves, or as a preliminary acquisition, minute descriptions of the several forms of matter, marks or tests by which they may be recognised, and names which may help us to remember them, and to classify and arrange them. The man who would be master of the art and science of construction (as architect or engineer) must have a preliminary knowledge of the descriptive sciences of geology and minerology. He must be able to recognise and appreciate the soil *in* which he builds, as well as the materials *with* which he builds; and in either case, the name by which he designates any soil or material is not, like the name of a star or a planet, used merely for purposes of recognition, as a man's name is, but as the representative of certain properties which it is not easy to identify. The difference between the two cases is, that when we deal with the *relations* of matter, we use names merely for the sake of *recognition*; but when we deal with the *properties* of matter, we use names for the purpose of *identification*. It is obvious that this circumstance introduces a new element of difficulty and uncertainty into all the arts and sciences which have to do with the *properties* and not with the simple *relations* of matter.

Another peculiarity in this class of arts and sciences is well worth noticing. I mean the wide variations in quality and consistence to which portions of matter bearing the same name, and even found in the same place, are liable. In speaking, for instance, of the strength of a given material used in building, as wood or iron, we are speaking of that which experience has shown to be subject to great variation of strength and tenacity; and experiments have accordingly to be made with a view to ascertain the mean or average strength of the material in question, as well as its extreme strength; and conscious of the great differences which may exist in this respect, the engineer, architect, or mechanician, is obliged to use an average value, or to resort to actual experiment on the material he is about to employ, when the scientific or practical man (the astronomer or the navigator) dealing solely with the *relations* of matter would be able to make use of an absolute numerical value. Here then is a second source of uncertainty and embarrassment, from which the more perfect sciences and arts are free.

I ought, however, to remark in this place, that even the astronomer is not wholly exempt from the necessity of resorting to a method of averages. He, too, must make some use of a numerical method, He has found out by experience that, though his instruments of calculation are perfect, the instruments by which he assists the sense of sight are imperfect, and his observations, partly for that reason, and partly from the variable condition of his own senses, are liable to certain slight errors both in excess and defect. To correct these errors, and to arrive as near as possible to the truth, he also resorts to that same method of averages which the engineer, the architect, the mechanician, the artilleryman, so constantly employ. The only difference is, that in the one case the method of averages is applied to observations; in the other, to experiments. Still, that part of the science of astronomy which consists in a record of such observations and their average results, is very small compared with the part which experiments and their mean results bear in rearing the science of the architect or engineer.

The elements of difficulty and uncertainty introduced into the science and art of construction by the necessary use of names which are subservient rather to identification than to simple recognition, and by the necessity of numerous experiments which can only issue in the establishment of average and extreme results—these elements of difficulty and uncertainty cling also to the science and art of *chemistry*. The names employed by the chemist are not simply names of recognition, but they are names of identification. Much of his time is taken up in ascertaining, by delicate manipulations and experiments, whether a substance submitted to him for examination possesses that aggregate of characters which a given name implies. And he, too, as we are all aware, constantly resorts to the average results of several experiments, because his experiments, like the observations of the astronomer, are apt to yield only approximations to the truth; and errors, both in excess and in defect, errors due partly to the imperfection of instruments, partly to the very place and time of the experiment, and partly to his own variable aptitude of hand and eye.

Hitherto, I have been speaking exclusively of the arts and sciences which deal with unorganised matter, or matter not endowed with life. I must now say something of those arts and sciences which have to do with living objects, whether belonging to the vegetable or to the animal kingdom. And first, of the science of botany, and the arts of *horticulture* and *farming*.

The science of botany may be said to consist of two parts—a part given up to the work of identification, and busied with names, descriptions, and classifications; and a part generally known as physiological botany, which is engaged in inquiries

into the minute structure, functions, and vital properties of plants.

Of the descriptions of the botanist, by means of which we are able to ascertain the true name of a plant, it will suffice to observe that they are of necessity extremely complicated as compared with the descriptions of inorganic bodies; inasmuch as plants consist of many different parts, such as the root, the stem, the leaf, the flower, the seed—each of which parts has to be so described as to be easily recognised; whereas inorganic substances have the same, or nearly the same, composition throughout, each fragment similar to every other.

Of the department of physiological botany, it may be observed, that it presents at least this peculiar difficulty when compared with sciences which deal with inorganic matter, that some experiments which are needful for the clearing up of disputed questions relating to the functions of plants are rendered unsatisfactory by the very circumstance that the subjects of the experiment are constantly undergoing vital changes which are interfered with by the experiments themselves.

The science of botany, in conjunction with the sciences of entomology, chemistry, geology, and meteorology, may be said to have its practical application in the art of gardening, and in that part of the art of farming which has for its object the culture of vegetable substances; and in these arts we see imperfectly set forth the peculiar difficulties which attach to the study and practice of the art of healing. We have to deal with living objects, over the growth of which we can exercise but limited control; while that process of growth is influenced by a vast number of different agencies as difficult to appreciate for purposes of science, as to bring under subjection for purposes of art. Over matter not endowed with life, we can exercise almost unlimited control. We can submit it to the operation of all kinds of force. We can crush it, melt or dissipate it by heat, dissolve or alter it by chemical agents, and transform it by the electric fluid. In short periods of time it undergoes no changes which we do not ourselves bring about. But these living things are in a state of perpetual change. There is a principle at work within them which we do not understand, and can only influence indirectly. From the sowing of the seed in the ground to the production of a plant in its turn bearing seed, external agencies liable to great and constant fluctuations are brought to bear, of which some are quite beyond our control, and others admit of only partial modification at our hands. Plants grown in the same soil, in the same climate, and with the self-same culture, are exposed to a new combination of the elements every year. No single constituent of that proverbially fickle and variable thing, the weather—neither the temperature, nor the fall of rain, nor the force and direction of the wind, nor the pressure of the atmosphere, nor the electric state of the air, presenting the same aggregate results in any two years, or even in any two seasons. Then there are the attacks of insects, and blights, and obscure diseases (such as those which have lately attacked the potatoe and the grape), occurring with strange irregularity; and still further interfering with the natural growth and perfection of the plant, baffling all the calculations of the scientific observer, and disappointing the most reasonable hopes of the gardener and farmer. All these things combine to render experiment difficult, and observation fallacious; and remind us forcibly of those analogous, but still more formidable and perplexing, difficulties which attach to the study of medicine as a science, and to the practice of the art of healing.

From the group of scientific studies which find their practical application in the arts of the gardener and farmer, the transition is easy and natural to the science of states and the art of government—to the abstract studies of the statist and the practical duties of the statesman. Between the work of the agriculturist and the work of the statesman there is an obvious and strict analogy. Each deals with events or results brought about by the operation of a multitude of causes acting with ever varying intensity and to a great extent beyond his control. Take, again, the case of the farmer. The seed he sows is of variable quality; the land he sows it in consists of a variable combination of soils; the mechanical preparation of the soil is more or less complete; no two specimens of the manure which he applies have exactly the same strength; and the crop grows up, arrives at maturity, is cut and harvested, each year under a new combination of atmospheric influences. From the same land, with the same preparation, the same quantity of the same manure, and, as nearly as he can judge, the same seed, he one year obtains an abundant, another year a scanty, crop.

So also with the scientific statesman. Relying upon the best information which he can procure, and basing himself upon the most accurate returns he can obtain, he makes some great financial change. But he is disappointed in the results, because he falls upon evil times—upon one of those cycles of years in which nothing prospers—or, he obtains an unwonted and unmerited success, because he happens to operate in a fortunate cycle of years. The unlucky statesman may encounter a bad harvest, or a series of bad harvests, a glut of foreign markets, a monetary crisis, an epidemic, rumours of wars, and popular outbreaks; and these things make themselves felt in the national balance-sheet, and insure the failure of all his calculations. His successor, on the contrary, when he attains to office may find everything taking a favourable turn. Abundant harvests at home and abroad, an active and remunerative commerce, an easy state of the money market, a sound condition of the public health, the best possible relations with foreign states, contentment among the people—all this, aided, perhaps, by the discovery of some new and unexpected source of wealth at home or abroad; and these things working together make all his plans to prosper, and all his reforms to seem the inspirations of genius. Like the physician, therefore, who gains credit where he does not deserve it, and loses it where he is not to blame, the statesman is often successful or unsuccessful through the concurrence of causes over which he can exercise no control.

In the sciences and arts which have to do with beings of a lower organisation endowed with life, such as the entire vegetable kingdom, and with the complex interests of human societies: in the science and art of agriculture and in the science and art of government, we see reflected, as in a mirror, but faintly and dimly, the difficulties and perplexities which surround the science of medicine and the art of healing. This important subject must be reserved for separate consideration.

[*To be continued.*]

Clinical Lecture

ON

OPIUM: ITS USE AND ABUSE.

BY

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[*Concluded from page* 315.]

The cases I have now brought before you, amply illustrate the wonderful efficacy of the narcotic power of opium. Opium soothes the nervous system, and renders it comparatively insensible to the operation of disturbing agencies, whilst by inducing tranquil sleep, it procures for the central organs that season of rest and repair, for which nature has provided in the state of health, by the periodical recurrence of night after the day, and without which those organs must undergo progressive wasting and degeneration. By restoring health to the nervous system, opium also indirectly invigorates the nutrition of the entire body; it restores energy to the functions of digestion and assimilation, renovates the muscles, and in so doing, strengthens the organs of circulation and respiration. It thus produces the effect of a tonic and a stimulant, and I have always thought that opium was indebted to this, its indirect action, for much of its stimulant property. It removes the depressing irritability and restlessness of exhaustion, whether induced by hæmorrhage, or by physical injury, and robs depressing influences, such as severe pain or intense cold, of half their power, by rendering the nervous system insensible to their operation.

I must now reverse the picture, and speak of the failure of opium. Like many other medicines, opium acts only in certain conditions of the system favourable to its operation, but such conditions appear to be of a very special nature. I put out of the question those constitutions which are entirely intolerant of its influence, though this circumstance constitutes a great obstacle to its general employment. But beyond such cases, there are many individuals in whom the medicine entirely fails either to soothe or to cause sleep. The stomach and digestive organs must be free from any great disorder, for opium to act favourably; this is a very important rule, and applies to opium as forcibly as to tonics; but there may be, and often is, something in the constitution of the nervous system itself, either natural or induced, which occasions resistance to the beneficial operation of the

medicine; just as excessive excitability is natural to some nervous systems, so an indisposition to yield to soothing influences is also natural to them. A state of considerable mental perturbation will often refuse to be calmed by opium, and even the same condition in a very minor degree; thus you may often observe, that patients, when first entering the hospital, will become sleepless for a time, in consequence of the new relations in which they are placed, and you will find that an evening opiate is often ineffectual in remedying the evil. I have even noticed a remarkable change take place in the nervous system, in its behaviour towards opium; a patient of mine, often the subject of wakefulness and of neuralgia, who uniformly derived the greatest comfort from an occasional dose of opium, now entirely fails in obtaining any relief from the medicine, even in increased doses; the pain is not abated, and the wakefulness is even increased; and a young gentleman, subject to fits of violent muscular excitement, who formerly relied upon opium, is now intoxicated by its influence, although the dose has been increased.

It is a well known fact, that in certain diseases, characterised by high excitement, opium is altogether inoperative; in tetanus, one thousand five hundred grains of solid opium have been taken in seventeen days. Dr. Chalmers (*Medical Observations and Inquiries*, i, 103), has given two hundred drops of tincture of opium at a dose; some patients, he adds, will take more than an ounce and a-half of laudanum in twenty-four hours, without experiencing its narcotic influence. Mr. Curling (*On Tetanus*, p. 56) mentions a case of tetanus, in which a drachm of laudanum was given every half-hour through the night, and sleep was not induced. During the passage of gall-stones, more than eight hundred drops of laudanum have been administered in less than twelve hours; and I need not refer more specifically to the effect of hæmorrhage in inducing tolerance of opium to a surprising degree.

Bearing these facts in mind, you will not be surprised at finding that opium often fails to induce sleep, in the class of cases with which we have been occupied; when the nervous system has been subjected, for a lengthened period, to the operation of sustained excitement, and when, moreover, the central organs have been lowered in their nutrition, and rendered thereby additionally prone to fall into an irritable condition, the calming properties of opium lose their effect. In a patient now in the hospital, who habitually disturbs those around him, by screaming at night, two grains of opium, given for a succession of nights, have not succeeded in gaining for him repose; an out patient, who has long been the victim of intemperate habits, has been taking two grains of opium nightly, for a considerable length of time, with very little benefit; and we have at present several other patients who have proved equally rebellious to the power of their medicine.

It happens, sometimes, that this state of unconquerable excitement and irritability attends the last days of certain individuals who have fallen into a state of great debility. I am now attending a lady, who is approaching her end, with symptoms of considerable depression of the power of the heart, in whom a state of excitement is very partially subdued by narcotics; and in one of the most painful deaths from degeneration of the heart, which I have witnessed, excitement amounting to mania, prolonged through twelve days, with nearly constant wakefulness, constituted a most distressing complication of the case. (*Provincial Medical and Surgical Journal*, 1852, p. 618).

Under circumstances such as those I have now mentioned, the question naturally presents itself, are we justified in meeting unusual resistance to the operation of our remedy, by a corresponding increase in its dose?

In attempting a reply to this inquiry, I have to present opium to you, from a different point of view, in relation to the injurious effects which it is capable of producing. The tendency of opium to constipate the bowels, to dull the desire for food, and to diminish the secretions, often creates inconvenience, though not sufficient to present any grave objection to its use. But there is another effect of opium of a far more serious nature; it undoubtedly exerts a directly and often very powerfully depressing influence upon the heart, particularly in cases where that organ has been already enfeebled by nervous depression, or by disease of its muscular fibres. The large doses of opium which patients will endure, when suffering from the collapse of hæmorrhage, or of severe injury, afford no proof that in conditions of far less exhaustion of the heart's power, induced by other causes, opium may be administered with equal impunity. I have seen a girl, poisoned by opium, perfectly pallid, and in such a state of collapse, that it required the support of two assistants to enable her to continue the perambulations necessary for her recovery; and the small quick irregular pulse of the first stage, the cold and pallid skin, the pale and ghastly expression of countenance of the second stage, enumerated by Dr. Taylor as among the symptoms produced by the poisonous action of opium, clearly evince the depression under which the circulation is labouring.

You will frequently find that patients who take opium to procure sleep, complain of nausea, even of vomiting, giddiness and faintness, when they rise on the following morning; occasionally, our hospital patients have suffered so acutely from these symptoms, that some persuasion has been needful to induce them to continue their medicine. I remember experiencing a striking proof of this sedative action of opium in my own person; having taken ten grains of Dover's powder, when suffering from an inflamed tonsil, I first experienced excessive nausea and sinking at the epigastrium; as these sensations increased, the pain and difficulty of swallowing were so entirely removed, that I firmly believed that an abscess in the tonsil had burst; the effect was precisely the same with that which had followed the previous application of leeches; unfortunately, it was even more transient.

M. Trousseau (*Medical Times and Gazette*, February 1859) has especially remarked upon this sedative action of opium upon the heart. He mentions two cases of heart-disease in which sudden death followed a single moderate dose of opium; and my friend, Dr. Johnson, has noticed the same circumstance.

The caution which is requisite, in the use of large doses of opium, in consequence of its possessing this tendency, has been observed by other pathologists in relation to the treatment of certain diseases. Thus Dr. Morehead (*Diseases of India*, ii, 638) objects to strong doses of narcotics in tetanus, because, associated with narcotics, there is always a failing action of the heart, and therefore, under the influence of narcotics, given in large quantities, the tendency to death by syncope, always distinct and important in tetanus, becomes seriously increased. In a case which occurred in Ireland, two years ago, and excited considerable attention at the time, death was distinctly referred, by a physician and surgeon of great eminence, to the debilitating effects, upon a heart already in a state of fatty degeneration, induced by large doses of opium, combined with full bleeding, for the relief of the pain of gall-stones (*Times*, November 19, 1857); and the opinion given gained greater importance from the uncertainty which the witnesses admitted to attend the early stages of cardiac degeneration.

The considerations I have now advanced must suggest caution in increasing the dose of opium, at least under the circumstances which I have mentioned; they suggest, also, the necessity of combining stimulants with the narcotic, and of enforcing the taking of food. You will often find that the mind, being preoccupied by painful thoughts, the stomach has felt little or no inclination for food, and that the feebleness natural to the disease has been increased by abstinence. It may, I think, be accepted as a rule, that wherever opium is required, stimulants will always be admissible, and will frequently be necessary; and with stimulants we must combine very careful attention to diet, administering food of a simple but nutritious character.

There is one disease which combines, in a very especial manner, the application of all that I have said respecting the advantage, the disadvantage, and the failure of opium; I mean delirium tremens. Among the symptoms of this disease, one of the most prominent, and certainly the most important, is obstinate wakefulness, which generally precedes the attack for some days, and continues through its duration. Wakefulness is at once a measure of the excitement, and a source of great danger. "If the patients pass many nights without sleep," says Dr. Watson, "they will die." Consequently, to procure sleep for the patient, and thereby to allay the excitement of the nervous system, and to recruit the energies of its central organs, becomes a leading principle in the treatment of the malady.

Now, delirium tremens very often affords most striking illustration of that obstinate resistance to the narcotic power of opium which I have already noticed; and if we are determined to procure sleep, we must not unfrequently be prepared to employ very large doses, and to repeat them frequently. Thus Dr. Morehead (*op. cit.*, vol. ii, 558) mentions one case of the disease in which two grains of opium were given in the evening; on the next evening a grain and a half of morphine; on the following day fifteen minims of laudanum every second hour, and in the evening two grains of opium every second

hour, for three hours; next morning three drachms of laudanum, and in two hours afterwards, two drachms more, were administered.

In another case, by the same author, a drachm and a half of laudanum was given in the evening; the following day half a drachm of solution of morphia every second hour, and in the evening, a grain of morphine. On the following day, two drachms and a half of laudanum; in two hours a drachm; and three drachms again in the evening, followed, in two hours further, by a drachm and a half. In neither case was any sleep, sounder than a doze, the result.

I treated two cases of the disease myself, on the same principle, some years ago, when in charge of a public institution, as resident officer; in both cases I repeated drachm doses of laudanum, every two or three hours, without success. In all these cases, due attention was paid to supporting the strength, by stimulants and by nourishment, but in all the patients died. In one of my own cases, the change from a state of high excitement to one of entire insensibility, with utter collapse, was fearfully rapid, and my notes of both the cases indicate evident signs of a tendency to prostration in the patients, by a feeble and rapid pulse, sobbing, respiration, tremulous limbs, and a surface bathed in perspiration. In a milder case, which occurred in the hospital the other day, you witnessed a similar failure of the opiate treatment. Now, in the history of delirium tremens, there is a circumstance of great moment in relation to the present subject—the great liability in the patient to degeneration of the heart, which, of itself, will sometimes bring the case to a suddenly fatal termination. To this I must add, that the secreting instruments of the kidney are rarely in a healthy condition. Various forms of advanced degeneration of the kidney are common results of intemperance; and, even when the disease has not attained this serious character, the epithelial cells will be found unhealthy, loaded with fatty and granular matter; as a consequence, the blood will probably be impure, and it is a well known fact, that impure conditions of the blood render the nervous centres more than usually exposed to the injurious operation of narcotic poisons. The extent of such changes will bear relation to the nature of the patient's habits, and to the length of time he has indulged in intemperance. Thus, in a first attack of delirium tremens, and in a tolerably healthy subject, but little anxiety need be felt for the result; the fit will probably subside of itself; whilst, on the other hand, when many previous attacks have occurred, the chances of recovery are much lessened.

Dr. Watson observes, that patients, in delirium tremens, "are apt to die much in the same way as patients who are poisoned by opium" (*Lectures*, 4th edit., i, 414). Now, when we recollect that what is commonly distinguished as apoplectic coma is sometimes induced by lowered nutrition of the brain, or by deficient supply of blood to that organ, it seems highly probable that such deaths are the consequence of failing power in the heart, more especially as we often find, after death, much increase in the subarachnoid fluid, together with pallid, shrunken, and widely-separated convolutions, evident signs of a wasted state of the cerebral tissue, a state most liable to suffer from any further depression in its nutritive conditions.

To apply these remarks to the case in question, I believe that it is better to introduce the element of time into our treatment of delirium tremens, and to endeavour to obtain the aid of opium, by administering it in moderate doses, but at regular intervals, whilst we combine with this plan of treatment all the aids we can obtain from quiet, darkening the room, cooling applications to the scalp, and the judicious employment of food and stimulants.

And I may add, that I think the regular administration of small doses of opium, at short intervals, will generally be more likely to be effectual, in obstinate cases of nervous disease, than single and powerful doses. We thus obtain the sustained operation of the remedy, which may succeed at last in overcoming the resistance opposed to it.

One word, before I conclude, on the employment of opium in fever. This is too wide a subject for me to do more than to make one remark in connection with it. We may sometimes notice, in the course of a single case of fever, or by comparing two cases with each other, two periods in the disease which present a very different, even an opposite relation to opium. In the early, and especially in the middle stage, we occasionally encounter a state of very wild delirium, approaching somewhat to mania; this happens chiefly in highly irritable, excitable subjects, and we noticed it in a girl, who died of fever some time ago, who had been exposed to a severe mental shock shortly before her illness; now, in this condition, we may meet with considerable resistance to the calming influence of opium. On the other hand, in the later stage, when the temporary support afforded by the febrile reaction has subsided, or when the patient has been much prostrated by the operation of the poison, and the muscular fibres of the heart are softened, the greatest caution is needed in giving opium, on account of the powerful effects it induces. I have seen a patient, under these circumstances, thrown into a deep sleep, lasting for many hours, by six minims of Battley's solution.

On some future occasion, I may speak to you of the different medicines which are employed as substitutes for opium.

Original Communications.

ON INTRAVASATION, EMBOLIA, AND CYTOSTASIS, AS CAUSES OF CEREBRAL AND OTHER PARALYSIS.

By J. L. W. Thudichum, M.D.

[*Read before the Medical Society of London, April 23rd*, 1860.]

The morbid conditions of the blood-vessels which, under the name of phlebitis and arteritis, had for many years been catalogued with typical inflammations, have during the last decennium received unexpected elucidation and a convenient nomenclature through the thoughtful labours of Virchow. It had no doubt many years ago been recognised by Cruveilhier that the coagulated masses found in veins, when in a state of so-called phlebitis, were not fibrine exuded from the wall of the vessel supposed to be inflamed, but coagula of blood simply, having no connexion with the contiguous membrane. The puriform substance, which is frequently found in the centre of these coagula, Cruveilhier continued to interpret as true pus, and admitted a migration of this pus from the membrane, on which it was supposed to have been bred, into the interior of the coagulum, by means of an unintelligible force termed by him "capillarity". The somewhat startling experience, that inflammation of the vein or phlebitis should always begin with a coagulation of blood in its lumen, caused the French pathologist to assume that all inflammations originally began with a coagulation of the blood contained in certain small veins, the existence of which was supposed, without further inquiry. Cruveilhier failed to work out the necessary deductions from his observations. Misunderstood and misinterpreted by many, his doctrines never obtained even general notoriety. For both the notice of Cruveilhier's somewhat mystical assumptions, and the true interpretation of his classical observations, the larger circle of medical readers is indebted to Virchow, who dissolved the doctrine of phlebitis, and substituted for it the expression of the simple fact, by arranging under the denomination of "thrombosis" those pathological conditions which consist in the closure of a vein by blood-coagula formed in the identical locality in which they are met with. The next step in this reform consisted in assigning its proper nature to the process by which a matter resembling pus was formed in the centre of these coagula. The differences from pus were in many cases so striking, inasmuch as the matter was often found destitute of the cellular elements characterising pus, and presented the granules simply of decomposing fibrine or decomposing blood-corpuscles, that even those cases where cells not distinguishable from pus-cells were found could be interpreted by those in which they were absent; in this sense, namely, that they were cases in which a thrombus consisting of coloured and colourless blood-corpuscles and fibrine had undergone the simple chemical process of softening; and that the fluid product was not pus, but *detritus*, the cellular elements of which were white blood-cells, which, by their greater durability, had outlived the destruction of the red corpuscles and of the fibrine.

The entrance of this *detritus* into the current of the venous blood had more frequently been assumed than observed. So long as it was confounded with pus, it was one of the principal factors in the theory of pyæmia, which, while almost entirely overlooking the possible mechanical effects of the entrance of this matter into the blood, attributed its main pathological importance to the poisonous nature of the liquid, which, by

engendering decomposition in the juices of the body, brought on toxic dissolution. But when Virchow proved that the entrance of fluid matter from softened thrombi into the blood was a much rarer event than the detachment from them of portions of coagulum destined to produce mechanical effects in distant places, and when he, moreover, proved that the entrance of such *detritus* into the blood from softened coagula in veins, or softened fibrinous concretions in the right heart, even when repeated, did not by any means produce the fatal disease known to surgeons under the name of pyæmia, it became necessary entirely to separate pyæmia from the effects of thrombosis and its immediate results, and to institute a new doctrine, to which its originator gave the name of embolia.

A case of embolia, then, presupposes an intravascular coagulum of blood or fibrine, formed in the vessel itself, and the detachment from that clot, by means of softening or mechanical action of one or several larger or smaller pieces, which are carried away by the blood-current, and thrown into distant vessels. On the other hand, we distinguish by the name of "intravasation" a process very similar in its effects, but different in its origin; namely, the entrance into the current of the blood of matters formed without the blood-vessels or in the substance of their walls, such as air or the contents of abscesses adjoining the vessels, the gritty matter of true atheroma, the calcareous plates and *detritus* of the tissue of degenerated arteries, or the pus from the circumscribed abscess occupying the wall, but leaving unaffected the lumen of larger blood-vessels, such as a vein in true phlebitis. In the heading of my paper, I have accused both embolia and intravasation as causing paralysis, and shall endeavour to substantiate the charge by the evidence of cases. I have joined with them the new name of a pathological process which has not hitherto been observed or distinguished. By "cytostasis" I desire to signalise the temporary or permanent arrest in capillaries or small arteries of blood-corpuscles, white and red, due to a peculiar glutinous condition of the blood, by which these bodies are enabled to adhere to each other with such tenacity as to form an effective bar to the arterial current of the blood. The word "kytos" (κυτος) for blood-corpuscle is indeed far-fetched, signifying a membrane, and next a membranous hold or bag, a cell; but you will excuse the application of the word in this instance, as it has been accepted in many others—to wit, cytoblastema, leucocythæmia, etc.—without any questions being asked about its origin. Of this cytostasis I have to bring before you a notable instance of great practical interest, inasmuch as it resulted first in temporary, ultimately in permanent hemiplegia, and was connected by a long train of symptoms with disease of the urinary organs. I have joined this case to several of intravasation and embolia, in which hemiplegia, or complete bilateral paralysis, paralysis of single organs, was produced; and hope that the variety of causes, processes, and terminations which these cases present, will make them deserving of consideration.

Case I. *Cytostasis.* A medical friend transmitted to me two specimens of blood which had been taken from a patient by cupping at the nape of the neck. The gentleman who operated said that he never met with blood of such consistency. It had more the thickness of liquid glue, and flowed so very slowly that it took an hour to remove what was sent to me from three places. The note continued to state that the patient, a gentleman of above sixty years of age, although living regularly and well, was of very spare habits; that for the last four months he had been subject to a most obstinate vascularity of the conjunctivæ of the eyes, which the treatment under one of our best oculists failed to remedy. Symptoms of loss of power on the right side had suddenly appeared. The patient had the greatest dread of depletion in any form; but the medical gentleman insisted upon its performance, and the patient derived marked benefit therefrom. In the opinion of this practitioner, there was a vascularity over the membranes of the brain, which would have resulted in apoplexy, had the cupping been omitted. He accounted for it by the inability of the blood to pass freely through the smaller ramifications of the vessels, and the production of cerebral compression.

On examination, the blood appeared extremely viscid, like a substance between tar and treacle. It was not coagulated, but contained small lumps of fibrine sufficient in quantity to enable me to say that it was not altogether absent. These lumps, under the microscope, consisted of granular matter densely studded with blood-cells. The blood had the ordinary colour, but became darker on standing. Under the microscope, the first feature observed was the abnormal appearance of the red corpuscles. Not only were they distorted in all possible shapes, but they also presented an extraordinary power of adhesion to each other, so as to form lumps and heaps, and the peculiar rolls, and could not be detached from each other by any manipulation. The next anomaly was the extraordinary number of white blood-cells, mixed with red ones; indeed, these two elements may be said to have been present in equal proportion, while in health one white cell accompanies about three hundred red ones. When the red corpuscles were made invisible by water, the white cells remained, mixed with many nuclei of white cells, which could also be traced in the blood not treated with water.

At my suggestion, the urine was now examined, and found to be albuminous. We also ascertained that, many years ago, the patient had been the subject of gonorrhœa and stricture, for which he had been salivated; anasarca appearing and going with the mercurial course.

The loss of power on the right side had repeatedly manifested itself before this last attack, but always again subsided. After the last attack, it became permanent; but the paralysis was for some time fluctuating in degree. When I saw the patient in consultation, all symptoms persisted; and I moreover found the presence of pus in the urine. All depressing and depleting measures were discontinued, and a tonic and diluent plan of treatment was adopted with benefit. When I last heard of the patient, his disorder had not made any unfavourable progress.

There is little doubt that the disease of the kidneys, produced by stricture and mercurial disease following after gonorrhœa, produced in an exceedingly slow manner a peculiar condition of the blood bordering upon leukæmia, and accompanied by an enormous increase of the solids of the blood and a viscous condition, whereby the blood-corpuscles not only passed slowly through the capillaries, causing vascularity among other membranes of the conjunctiva, but also adhered to each other, forming lumps, whereby some smaller arterial branches were, at first for a time, ultimately permanently obstructed, and paralysis was produced, probably not by compression, but by anæmia of some motor portions of the brain-substance.

Case II. *General Paralysis caused by Intravasation.* A gentleman, aged 59, came under my care for slight disturbance of the digestive functions. Six years before, he had had an apoplectic attack, followed by paralysis of the left side. Coetemporaneously with the occurrence of the apoplectic seizure, a tumour of the size of a fist, which the patient had for several years carried above the left clavicle, had suddenly disappeared, without any external opening having been effected. Subsequent to the first attack, the patient suffered from time to time from convulsions, which came on particularly during the night, and always left him worse. Breathing became so difficult that he had to sleep in a sitting posture. The right side also began to get weak, and soon became as palsied as the left. Six months before I first saw him, after another attack of convulsions, he had entirely lost his speech; and, with the exception of his eyes, he could in no part of his body perform the slightest voluntary movement. Yet he was perfectly conscious, felt pain, and saw and heard, as his eyes interpreted to the examiner by the performance of prescribed movements. The paralytic condition of the larynx and pharynx at last constantly threatened suffocation. The urine became albuminous, having been normal before; and the patient rapidly emaciated. The excretory functions remained unimpaired to the last. To such a sufferer, death was mercy.

The *post mortem* examination showed an atrophic condition of the dura mater. Between this very thin membrane and the arachnoid, there existed a considerable collection of serum, amounting to five ounces of fluid. The arachnoid was turbid, opaline, thickened, and easily separated from the brain-substance. The arteries of the arachnoid showed numerous white deposits in the substance of their walls. When an artery was slit open, the inner membrane could easily be separated from the outer; and the white deposits did not adhere to either, but could be perfectly enucleated. They consisted of a material of the hardness of fibrous cartilage, but were found to be fibrous only under the microscope. The hemispheres of the brain exhibited at their outer layers the condition of atrophy (by pressure?). A cortical layer, of the thickness of one-sixteenth of an inch, yellow and firm, was succeeded inwardly by a gelatinous glistening layer of about one-eighth of an inch in thickness, and upon that followed normal brain-substance. No vestiges of any extravasation could be discovered. The lungs exhibited numerous black spots, distributed all over

their surface, which to the touch appeared as granules imbedded in the tissue. Several were enucleated. The largest were the size of a pea. They consisted of amorphous black matter, being actually disintegrated blood, the residue, no doubt, of former extravasations. The abdominal walls contained an abscess under the peritoneum, probably of recent origin. The kidneys were in a state of fatty degeneration.

I dissected the region above the left clavicle where the tumour had been, which, at the time of the apoplectic seizure, had suddenly disappeared. Behind the clavicular portion of the sterno-cleido-mastoid muscle, just underneath the fulcrum of the omohyoid muscle, in the angle formed by the junction of the internal jugular and subclavian veins, lay a tumour of the size of a large walnut, firmly attached to the walls of the veins. The internal surface of the veins, corresponding to the parts in adhesion outside, were irregular, with several obliterated openings, and some fibres crossing the lumen of the subclavian vein. These appearances represented the residues of former perforations. The tumour was a fibrous husk, filled with thick, inspissated, and degenerated old pus. It no doubt represented the residue of the tumour, the disappearance of which accompanied the onset of the paralysis.

I have subjected to a most careful scrutiny all the anatomical detail of the case, and I have come to the conclusion that the paralysis was the consequence of the entrance into the cavities of the subclavian and jugular veins of pus contained in the abscess. This pus and *detritus* had passed with the blood to the heart, and thence was driven into the lungs. Here its first effect was to make the capillaries impervious; the heart becoming distended to its utmost on the right, empty on the left, would exert a great pressure backwards into the veins, and forwards into the pulmonary artery. Hence the capillary apoplexy in the lungs, which had left the black deposits of changed blood. The pressure in the venous system produced the symptoms of apoplexy by compression of the brain and subsequent serous exudation.

Case iii. *Embolia.* I will shortly refer to a case of embolia, somewhat similar in its effects, which I read before this Society on March 26th, 1856, and which is published in the Association Medical Journal for April of the same year. An elderly female died of the consequences of kidney-disease, its stages modified in a remarkable manner in the left kidney. There was eccentric hypertrophy of the whole heart, producing fibrinous clot in the right auricle, softening and discharge of the *débris* into the lungs. Mechanical stasis and hepatisation of the lungs, with expectoration of blood, ensued; the pressure in the venous system produced œdema of the arachnoid, with convulsions and paralysis. In the heart was found the husk, hollow, and containing yet some detritus, of the fibrinous concretion. Detritus was discovered in the finer ramifications of the pulmonic artery.

Intravasation of air produces symptoms very similar to those of extensive embolia of the lungs. The entrance of air into the veins, in fact, kills by interruption of the pulmonic circuit, consequent pressure in the veins, with pressure upon the brain, and ultimate exudation or extravasation. Upon this point, I have made direct experiments by blowing air into the jugular vein of horses. The pressure in the venous system rises to an extraordinary degree.

Case iv. *Arterial Intravasation.* A boy, aged 11 years, came under my care with symptoms of shifting rheumatism. He complained of pain in the left side, underneath the heart, which he ascribed to ill treatment from another boy. The pain shifted to the back; the left hand became swelled and painful, the left foot having been so a little before. I diagnosed rheumatic fever, with a query. On the fifth day, pain in the region of the bladder came on, with occasional changing. All outward symptoms disappeared, as well as all internal objective symptoms, with the exception of the high fever. The heart was expressly examined, and found healthy. Two days later, there was systolic murmur in the left heart.

The boy complained of constant pain in the belly. The scrotum was slightly œdematous, his feet and legs cool. The pain in the belly he also ascribed to ill treatment from another boy, who, he said, had kicked him all over his body while on the ground. He continued to fever, and died one night rather suddenly, all medical treatment notwithstanding.

On *post mortem* examination, I found that death was due to hæmorrhage from rupture of the aorta at its bifurcation, under the following circumstances. There existed what appeared to be an abscess of the right psoas muscle, in the cavity of which the right iliac artery terminated, being obliterated on the other side of the cavity. The left iliac was similarly enlarged, but more in the manner of an aneurism. Both these cavities involved the bifurcation of the aorta, and had corroded and thinned the wall of the ascending cava. There was endocarditis, evidently of recent origin, and an abscess in the spleen.

The interpretation of this case was connected with great difficulty, as, owing to the continually repeated statement of the ill-treatment received, it became the subject of a coroner's inquiry. Abscess of the psoas muscle being a well-known idiopathic disease, due to causes not known, has also been observed as a consequence of violence applied to the abdomen. Aneurism, of a traumatic nature, has also been observed after kicks upon the belly, and the bifurcation of the aorta, situated as it is on the most prominent part of the spine, is a very likely part to be injured by such a cause. That the pseudo-rheumatic symptoms were due to pyæmic infection of the blood, I am inclined to believe. The case is open to different constructions in some respects, but the fact remains that an abscess and an artery communicated, that detritus was carried into two large arteries, which were thereby obliterated, almost entire paralysis of the lower extremities and sinking of the temperature being the consequence. It is not difficult to comprehend the infection of the blood to have been effected by the entrance into the iliac artery of fluid pus before its obstruction with *detritus*, and the return of this fluid with the venous blood to the general circulation, there to effect the swelling of hand and foot, the endocarditis and the abscess of the spleen.

The œdema of the scrotum is interesting in a diagnostic point of view, as it was caused by the compression of the seminal veins, which passed across the most prominent points of the abscesses. From the same cause the ureters down to the places of disease were considerably enlarged, and filled with urine.

Case v. *Arterial Embolia depending upon Heart-Disease.* This I have observed in several cases. In one, that of an elderly lady, the heart disease was in apparent connection with former dropsy, from nephritis. A clot formed in the left heart was carried into the left crural artery. The left leg was quite cold, like a dead limb, and benumbed. The toes could not be moved. The leg became dark, and blistered, gangrene setting in, at the beginning of which the patient succumbed.

Case vi. *Entrance of Atheromatous Matter and Calcareous Plates into the Arterial System.* I have observed the entrance of atheromatous matter and calcareous plates into the aorta of an elderly female, who died of somewhat sudden effusion of serum in the abdomen, connected with the effects of gastric ulcer upon the liver and portal vein. At the *post mortem* examination, a calcareous plate was found arrested at the bifurcation of the aorta, where it had been carried from a corroded portion of the thoracic aorta.

It deserves to be pointed out, that in two of the cases above related, the kidney disease was unaccompanied by dropsy in its later stages, which produced the paralysis. In one case the kidney disease was the consequence of the general paralysis. In two cases, the heart disease giving rise to clots, was due to kidney disease. And in the case of the elderly lady who died of gangrene, after embolia, no kidney disease could be discovered by examination of the urine, although she had been dropsical years ago.

In the cases related, the paralysis was mainly confined to the motor nerves, or was caused by the absence from the muscle of arterial blood. In most cases sensibility was only impaired, not absent.

The absence of extravasated blood from the brain is also a remarkable feature, proving that the blood vessels of the brain have not, or only little, tendency to rupture, unless they are in an idiopathically diseased state.

The case of cysostasis is closely allied to those cases of pseudo-fibrinous clot formation, where the clots are found to be made up of white blood-corpuscles mainly. These corpuscles are always very glutinous, as is evident from the manner in which, in the frog at least, they adhere to the walls of capillaries. But in the above case analogy is hardly needed to illustrate the direct observation.

I have thus laid before you results of the use of the scalpel and microscope mainly, results however which I hope will appear of some practical usefulness by combination with the clinical history of the cases. All new doctrines require frequent rectification and revision. And if to the doctrine of intravasation and embolia I have been able to add the novelty of cytostasis, I hope that my lucubrations will not be considered altogether unprofitable.

Transactions of Branches.

BIRMINGHAM AND MIDLAND COUNTIES BRANCH.

OBSERVATIONS ON VIRCHOW'S THEORY OF INFLAMMATION.

By FRANCIS T. BOND, M.B., late Physician to the Queen's Hospital, Birmingham.

[*Read April 19th*, 1860.]

WHEN a new theory of a phenomenon which has occupied the attention of observers from the earliest history of medicine is propounded by one who speaks with the authority of the professor of pathological anatomy in the University of Berlin, it is not only natural, but necessary, that it should receive a more searching criticism than if it had proceeded from a less authoritative source. In his recent work on cellular pathology, Professor Virchow has thrown down the gauntlet to the scientific world on more than one subject of physiological importance, and amongst others on that of inflammation; and as, notwithstanding the novelty of some of his views, they do not seem to have attracted in this country the attention which might have been anticipated, I have thought that a half hour of the Society's time might not be unprofitably spent in discussing the theory which he has propounded on inflammation in general, and more particularly on the relation which exists between the local exudation and the general blood-disorder in inflammatory diseases.

Virchow commences his remarks upon the subject of inflammation by a short historical review of the changes which have successively taken place in the views of its pathology, and shows how they have been generated by the predominant importance which at one time or another has been attributed to the old-established symptoms—"heat, pain, redness, and swelling". He points out how the former of these appeared to Galen and the ancients the weightier element, whence their employment of the terms "inflammatio" and "pyrexia", as expressive, not metaphorically, but really, of the living fire in which the essence of the affection appeared to consist. How with the progress of pathological anatomy the increased vascularity of the inflamed part attracted notice; and how, in France especially, under the influence of Broussais, the hyperæmia so effectually diverted attention from its concomitant phenomena as to have been frequently put forward as synonymous with inflammation itself. How, after another turn of the wheel of speculative opinion, we find one branch of the reactionary Vienna school (the neuropathologists) tracing the etiology of inflammation to the "pain" as a starting point, and subordinating the anatomical and visible phenomena to the unseen and nervous ones; whilst a second branch of the same school, followed by some distinguished writers in our own country, swear as positively by the "swelling", or "exudation", as the *sine quâ non* of the disease. Now if, as Virchow says, we must choose between these four symptoms one which shall be considered the most constant and typical, as physiologically and practically the most important, there is no doubt that the exudation best deserves our choice. It is less frequently absent than either of the others; it is less ambiguous when present; and it is more indicative of an organic change of the activity and functions of the part affected than they are. But to demand the presence of an exudation, especially one of a fibrinous character, as the only positive evidence of inflammatory action, is a very different matter; for, as Virchow proceeds to remark, although inflammation of the alimentary mucous tract is frequently accompanied by a copious exudation, it is one of a mucous and not a fibrinous character. In pure hepatitis, and in encephalitis too, a fibrinous exudation is rarely or never produced. On the other hand, the disturbance in the functions of an inflamed part may result in the production of either pus, mucus, or fibrine, or of substances intermediate between them. Now if the advocates of the exudatory theory will so far widen the limits of the term *exudation* as to include *pus* and *mucus*, then an exudation may be considered as the only evidence of a completely established inflammation; for few will be bold enough to assert that an inflammation can exist which does not include the presence of pus, mucus, or fibrine, as one of its extravascular results, and probably fewer still will attempt to demonstrate it. This point being settled, however, a fresh difficulty arises. If the term exudation is to be stretched to include pus and mucus, how far is it a correct designation for these products at all? An exudation, in the ordinary acceptation of the term, is something that exudes, or passes out, as such, from the blood. We know that pus does not naturally exist in the blood, and we have no proof that mucus does; they, therefore, cannot strictly be called exudations, but are developments from extravascular processes, though indirectly exudative products. But if pus and mucus must be considered as results of extravascular histogenetic processes, may not fibrine also, which has generally been regarded as *par excellence* the material of exudations and a blood product, and which is inseparably connected with mucus and pus, be itself no exudation, but as much an extravascular formation as they are? It is clear that if this view can be established, the original meaning of the term exudation must be abandoned; for if the so-called exudation consist for the most part of fibrine, and the fibrine is manufactured in the tissues, it is no exudation at all, but only an increased production of a substance which it is the natural function of the tissues to create; though certainly such an increase of production must necessitate an increased supply, or exudation, if we may so say, of nutritive material from the blood. This is the view which Virchow adopts, and we cannot therefore be surprised at his saying, "I hold, that in the sense in which the term is generally employed, there is no exudation in inflammation at all, but that the so-called exudation is really the material which is developed in the (tissues of the) inflamed parts, as a consequence of their altered function, from the fluids supplied to them by the vessels in their vicinity".

Now, in addition to the local development of fibrine in the exudation, all inflammatory diseases are accompanied by an increase of the fibrinous element of the blood, an increase which varies in different diseases, but which bears a pretty constant ratio to the increase of temperature which attends the inflammation, and to the increased secretions by which it is followed. What is the relation of this hyperinotic, or hyperfibrinous condition of the blood to the local exudation? Is the former produced by the latter in virtue of a sort of reaction of the local affection upon the general mass of the blood, or is the exudation an elimination of the excess of fibrine pre-existing in the blood into and upon the tissues amongst which it is found? The determination of this question is important both in a pathological and a therapeutical point of view; for if the hyperinosis is the *fons et origo mali*, and we can, by therapeutic measures, in any way check or destroy it, we are stopping the enemy's progress in his local attack, by cutting off his supplies. If, on the other hand, the hyperinosis is the *result* of the local affection, then it is equally clear that all our efforts should be directed to the latter quarter, and that on the cessation of the local affection the general one will disappear of itself. Virchow embraces the latter of these views, though in a form different from that in which it is usually accepted. Instead of regarding the fibrine of the blood as an intravascular product, evolved directly from the materials supplied by the food, or produced by the metamorphosis of other elements of the blood, and destined to the nutrition of the tissues, he considers it to be developed by the tissues, especially by the cellular tissue, and to be as completely extravascular in its origin as pus or mucus, only that, unlike them, it is absorbed into the blood by means of the lymphatic system. In those cases of local inflammation which are accompanied by an increase of fibrine in the blood, he considers that increase to depend upon the increased activity of the secreting functions of the tissues of the inflamed organ, the fibrine produced by them being carried in large quantities into the blood, and giving rise by its accumulation there to the well known hyperinotic condition of inflammatory blood.

It may not, perhaps, be altogether a waste of time if we briefly run through the grounds upon which Virchow bases these views. For, although Zimmerman and others have previously advocated the idea of the fibrine of the blood being a waste product, and not one adapted for histogenetic purposes, no one, so far as I am aware, has so distinctly formulised the doctrine, or applied it to the process of inflammation, as Virchow.

He commences by pointing out that there are two distinct varieties of fibrine, one which exists in the blood, and the other which is found in the lymphatics. The former of these not only coagulates after death in the vessels, but may do so during life; it also coagulates on exposure to air. The latter never coagulates in the vessels either during life or after death; it does so on exposure to air, but much more slowly and imperfectly than the fibrine of the blood, which, however,

after coagulation, it appears to resemble. A substance resembling the lymphatic fibrine is occasionally found in the exudation of pleurisy, which coagulates spontaneously on evacuation. It is to its presence, also, that Virchow attributes the white crust of inflammatory blood, which has been called *crusta pleuritica*, on account of its being so especially evident in the blood of pleuritic patients. The increase of fibrine in the blood is greater in inflammation of certain organs than of others; and such organs are largely supplied with lymphatics, *e.g.*, the lung. Some organs, on the other hand, may be extensively inflamed without giving rise to a hyperinotic condition of the blood, *e.g.*, the brain, which possesses hardly any lymphatics. From these two facts, Virchow deduces the conclusion that the fibrine of inflammatory blood is dependent for its origin upon the inflammation of some organ which is richly supplied with lymphatics, by which the fibrine secreted in the tissues is carried into the blood. It does not therefore follow, he says, as has been asserted by some authors, that because a man has an excess of fibrine in his blood he has any tendency to the development of local inflammations; but the local production of fibrine in any organ (exudation) will lead in time to a hyperinotic condition of the blood, the general hyperinosis being the excess of the locally-developed fibrine. He points out, as a corroboration of his view as to the relation of the fibrine and lymphatic system, that we never get an increase of fibrine in the blood, without at the same time finding an increase of the white corpuscles—bodies which appear to be formed in the lymphatic system.

With regard to the local exudation, Virchow asserts that no local development of fibrine, or exudation, can take place unless some stimulus external to the circulation has been applied to the spot, and he shows how the character of the exudation depends upon the functional activity of the inflamed part, or of the system generally; thus a blister, when applied to the skin under ordinary circumstances, causes the formation of a bulla of fluid, which only coagulates after exposure to the air; whilst in certain conditions of the body, or part, a similar bulla of spontaneously coagulating fibrine may be produced. In the same way he explains the occurrence of two varieties of pleurisy, in one of which the exudation is spontaneously coagulable (*lymph*); in the other it is not (*serous*). The occurrence of an exudation is not, however, essential to an inflammation; it can only be produced in parts which are well supplied with vessels, from which the exudation is supplied. In such parts hyperæmia always precedes the exudation, and may therefore be practically considered as a characteristic symptom; but in parts which are wholly destitute of vessels, or but poorly supplied with them, as the cornea, cartilages, tendons, etc., neither hyperæmia or exudation can occur, and the inflammatory process is limited to insterstitial changes which take place in the tissues themselves, *e. g.*, the production of pus. Taking his stand upon these facts, Virchow reduces all the varieties of inflammation to two categories; the *parenchymatous*, where there is no exudation, ordinarily so-called, and the phenomena of inflammation are limited to interstitial changes occurring in the parenchyma of the tissues; and the *secretory* or *exudative*, in which an exudation from the blood furnishes the materials for histogenetic processes, the results of which are often eliminated with a portion of the exudation upon the surface of the organ, in the form of a copious secretion.

It will appear from this brief statement of Virchow's views, that in his opinion the process of inflammation originates neither in the blood nor the blood-vessels, but in the extravascular tissues, whose affinities for the plasma of the blood are exalted to such an extent as to attract an increased amount of material from the vessels in their neighbourhood, in the form of an exudation. Consequently, the commencement of the process must be sought for in the stimulus or influence by which the natural affinities of the tissues for the blood are so largely increased. "When we speak", says Virchow, "of this cause—or stimulus (*reize*)—of an inflammation in any part, the only exact idea we can form of it is, that through some influence exerted upon that part, either from without or through the blood, its condition and functions undergo such alterations as to immediately affect its relations to neighbouring parts, and to place it in such a position that it abstracts from the organs in its immediate locality, whether vascular or not, a greater quantity of material than usual, and appropriates it to its own structures. Every form of inflammation with which we are acquainted can be explained upon this hypothesis; and all agree in this, that they commence from the moment that this increase of material is supplied to the tissues, and that they are accompanied by an increased assimilation of that material."

As to the immediate nature of the stimulus which determines this exalted activity of assimilation on the part of the tissues in inflammation, Virchow, in the present work, expresses no opinion. That it is, in many cases, intimately connected with nervous action is certain, as is shown by the effects of lesion of the sympathetic; but as inflammation occurs sometimes in the tissues, which are, as far as observation allows us to judge, unsupplied by nerve-fibres, it would seem that in these cases the stimulus must be identical with a heightened condition of that natural irritability of the tissues themselves which they possess independently of nervous influence. It is in the recognition of this increased attraction of the tissues for the elements of the blood, in inflammation, that the merit of Virchow's views consist. Nothing can be more certain to any one who reflects carefully upon the nature of the process of inflammation, than that we have in it to deal with two elements—the disturbed functions of the blood and vessels on the one side, and of the extravascular tissues on the other. It is in the onesidedness of their views that the advocates of what may be called the vascular congestion, or exudation theory, err. They see but the hyperæmic state of the inflamed part; the congested vessels, and the arrested circulation, terminating in an exudation of plastic matter; and seizing upon the latter of these phenomena as the culminating point of the series, they endeavour to explain it by the very mechanical hypothesis that the congested vessels seek to relieve themselves of their contents by pouring out an exudation into the neighbouring tissues. They omit all reference to the fact that every part of the animal organism is itself a centre of nutritive activity; that every cell is developed and maintained in its growth in virtue of the attractive power by which it draws from the general nutritive fluid the materials adapted for its own support; and that it is impossible to establish any line of demarcation between the ordinary nutrition of the tissues and that hypertrophy of them by which the inflammatory process is accompanied, and in which it appears chiefly to consist. How difficult is it frequently to say whether a thickened fibrous membrane is the result of chronic inflammation or chronic hypertrophy; or an enlarged bone; or an enlarged gland? All these effects may be produced by inflammation; but they also equally may be produced by a simple increase of the ordinary process of nutrition. In ordinary nutrition we have two forces to deal with; the fitness of the elements of the blood for the nourishment of the tissues on the one hand, and the power of appropriation or selection possessed by the tissues on the other;—a *vis a tergo* on the part of the blood, a *vis a fronte* on the part of the tissues. Of these two forces, it will be evident, from the nature of the case, that the attractive power of the tissues must be the more potent, since the blood is destined to nourish the tissues, and the tissues are not created to relieve the blood of its superfluous material, though they do secondarily perform that office. Now if we have this double play of affinities to deal with in ordinary nutrition, it almost necessarily follows that they should also exist in that extraordinary or tumultuous nutrition in which inflammation consists; and that if the attractive power of the tissues—the *vis a fronte*—is the more active agency in the regular or healthy process, it should also be the primary and more potent one in the exaggerated or diseased condition. Here, then, we see the force of Virchow's reasoning when he attributes such importance to the "stimulus" or exciting element of inflammation, and when he seeks to place it, not in the blood or the vessels, but in the tissues. In another place (On the Pathology of Rheumatism, *Midland Journal of Med. Science*, April and July 1858) I have pointed out how the omission of these considerations has led to a misconception of the nature of some inflammatory diseases, and especially of rheumatism, though I have endeavoured also to show that the hyperinotic condition of the blood in those diseases is a primary cause, and not a secondary effect of the affection. In this respect it is obvious that the views which I have advocated are not in accordance with those of Virchow; and although his theory is so satisfactory as a whole, and the grounds which he adduces to support it are so powerful, there are some facts which appear to me inconsistent with it, and which I will briefly mention.

In the first place, Virchow asserts that the fibrine of inflammatory exudations passes into the lymphatic system, and is distinguishable from that of the blood by not being spontaneously coagulable; but the fibrinous material of inflammatory exudations is always not only coagulated, but organised; it is therefore difficult to see how it can be identical with that of

the lymphatic system. On the other hand, it resembles in all its properties the fibrine of the blood, from which, therefore, it is only fair to infer that it takes its origin. Again, microscopic observation of the phenomena of inflammation, as they exhibit themselves in the web of the frog's foot, shows us that the stagnation of the capillary current produced by the application of a local stimulus is followed by an evident exudation into the neighbouring tissues, which exudation assumes gradually a fibrous organisation. It is clear, in this case, that the production of fibrine in the exudation is quite independent of the agency of the lymphatics, and that it is an automatic act of the exudation itself.

It is in the phenomena of acute rheumatism, however, that we find the greatest difficulty in reconciling the theory of Virchow, as to the origin of fibrine, with facts. We know that in this disease the increase of fibrine in the blood, the intensity of the fever, and the increased amount of solids in the urine, go together; and there can be but little doubt that the two latter are dependent upon the former. Now it is by no means an uncommon clinical fact for both of these latter phenomena to exist in considerable intensity, and yet for no local symptoms of inflammation, either in the heart or joints, to be present; and even when they are, it is by no means a rule that the intensity of the local and general symptoms progress *pari passu*, the latter being frequently out of all proportion to the former. If Virchow's theory were correct, and the increase of fibrine in the blood, and consequently the production of fever and its concomitants, were dependent upon the local affection, it is clear that the intensity of the general pyrexia should be entirely dependent upon the extent of the local inflammation; and not only so, it should never in point of time precede it, which it sometimes unquestionably does. For although in many cases the first indication which the patient gets of the disease is the local pains, which are popularly supposed to demonstrate its existence, yet there are others in which for some time the only affection present is general fever, which at last appears to concentrate itself upon some one spot. Nor can it have escaped notice that occasionally, where the general pyrexia has been pretty intense, the development of pericarditis or pleurisy, or a sharp attack in the joints, is followed by a diminution in its intensity, to such an extent that the patient often expresses himself as greatly relieved. Does it not appear as if the largeness and rapidity of the exudation, in these cases, acted as a safety valve to the general affection? Such an explanation of the relationship between these two phenomena may be considered somewhat metaphorical, but the idea is certainly a standing one amongst clinical observers.

There is another feature of inflammatory diseases which Virchow appears to ignore, and which his theory does not explain, viz., the inverse ratio which exists between the fibrine and the red corpuscles of the blood, the latter always diminishing in number when the former is increased. The uniformity with which this relation obtains cannot but lead to the inference that these phenomena are mutually dependent upon one another, and that the increase of the fibrine is owing to the destruction of the blood-corpuscles. Many facts, into which it is impossible to enter here, lend support to this hypothesis, and if it be correct, it is evident that we must look to the circumstances which affect the vitality of the blood-corpuscles, or which facilitate their dissolution, for the predisposing causes of inflammatory diseases. It is somewhat singular that Virchow, in whose theories the universality and importance of cell-growth appears to be the leading principle, should have omitted so favourable an illustration of his views as the supposition of such a connection between the red corpuscles and the fibrine presents. The practical importance of the question, as to the origin of fibrine, will be obvious when its bearings upon such a disease as rheumatism are considered. If the hyperinotic element be the "head and front" of that disease, as I think may be shown without difficulty, whatever theory as to its pathology we may assume, and if that increase of fibrine depend solely upon the activity of the local affection, as Virchow would have us believe, it is clear that all our therapeutic efforts should be directed to the latter quarter, and all constitutional methods of treatment may be safely omitted. If, on the other hand, the increase of fibrine in the blood be the original disorder, giving rise to the local one secondarily, either by the natural affinity which exists between the fibrous tissues, which are the ones chiefly affected in rheumatism, and the fibrinous element of the blood; or by the metamorphosis of the excess of fibrine in the blood giving rise to the formation of such products as uric and lactic acids, which may act as irritants upon the fibrous tissues, and so set up inflammation in them—under either of these hypotheses, it is to constitutional treatment that we must look, not only in the alleviation of the acute attack, but also for the means of preventing a recurrence of the disease. The plan of treatment which is generally adopted in the attack of acute rheumatism, in this country, is certainly directed more to influence the hyperinotic state of the blood than the local symptoms. For, putting out of question such therapeutic agents as are employed for special purposes, such as the relief of pain, the increase of the excretions, or counterirritation, the general plan of treatment in acute rheumatism must be admitted to be by salines; and whether those salines have the character of neutral salts, such as the nitrate of potash, or of alkaline ones, such as those of the carbonates and vegetable acids, their influence, from all that we know of their physiological action, must be exerted primarily upon the blood, and chiefly on its fibrine. So, too, in the treatment of the chronic form of rheumatism in which the pathological conditions are the same as in the acute form, the elements of time and intensity alone omitted, it is very difficult to deny that the beneficial effects of cod-liver oil are dependent upon the power which it possesses of physiologically neutralising, if we may so say, the excess of albuminous and fibrous elements which the blood must contain, and by that means restoring to it its healthy equilibrium.

Even if it were not easy to show, as in the case of rheumatism and similar diseases, that this question of the origin of fibrine has a directly practical importance, it would not be the less a question deserving the consideration of every intelligent practitioner. For as no practice can be efficient which is not founded upon enlightened and comprehensive views of pathology, so no pathological system can be complete, or even rational in its bearings, which does not profess to give at least some plausible explanation of the origin and destination of the principal proximate constituents of the animal fabric. In the present imperfect state of our knowledge on the elements of the blood, it is impossible to be too careful in giving more than a hypothetical weight to any views which may be advanced as to their physiological or pathological relation to one another. But it is equally clear that no doctrines, either of physiology or pathology, can ever be generally established until they have become familiar to the mind of the medical public at large, and have been subjected to that practical and protracted examination which the observation of disease by every intelligent practitioner is calculated to afford. These are the reasons which have induced me to occupy what I am afraid has been an unusually large share of your time this evening with these remarks on the theory of Virchow; and though it is a subject, in its present stage, rather for reflection and observation than discussion, I hope that it has not been entirely out of place on the present occasion.

SOUTH-EASTERN BRANCH: ROCHESTER, MAIDSTONE, GRAVESEND, AND DARTFORD DISTRICT MEETINGS.

TWO CASES OF NARCOTIC POISONING; SUGGESTING A MODIFICATION IN THE USE OF THE STOMACH-PUMP.

By J. Evelyn Crook, M.D., Northfleet.

[*Read at Gravesend, March* 30, 1860.]

Gentlemen,—I purpose reading the details of two cases of narcotic poisoning, one in a very young infant, the other in an adult. The reason I consider these cases worthy of your notice is, because they prove that it is not necessary to *pump* fluid from the stomach, any more than from the urinary bladder, as both viscera may be emptied merely by passing a tube.

Case I. May 15th, 1857. An infant, only twenty-two days old, had about forty minims of laudanum poured down its throat. The occurrence happened in this way. The mother, after her confinement, was attacked with muco-enteritis; and, among other remedies, an opiate injection was sent to the house, part of which only was administered, as the patient quickly recovered. It appears an elder child, seven years of age, was left alone with the infant, and she deliberately took the bottle containing the remainder of the opiate injection from the mantel-piece, and poured its contents down the infant's throat. The accident was soon discovered, and I was sent for; but not being at home at the time, did not arrive till the poison had been taken about half an hour.

When I saw the child, two mustard emetics and some antimonial wine had been given (how much, I could not ascertain), but no vomiting had taken place. The pupils were contracted; the skin was cold; indeed, the child appeared to be dying. I immediately sent for my elastic male catheters, and passed a large one (No. 12) down the œsophagus. I had no sooner done so, than the contents of the stomach flowed through the instrument, and were collected in a basin. I filled my mouth with warm water, and by applying my lips to the end of the catheter, forced the water into the stomach; the fluid was immediately returned through the tube. This was repeated several times. I afterwards injected some warm coffee, and withdrew the instrument, the child being all this time in the upright position. The little patient was then enveloped in flannels wrung out in water tolerably warm; and at frequent intervals cold water was dashed over its body. This treatment was continued many hours, the infant during the time being fed with milk and brandy. Twenty-four hours elapsed before it was again able to take the breast. It recovered, and is now alive and well.

Case II. On the 23rd of last November, a discharged soldier from India had been lying at the railway station in a state of coma for seven hours. At first, he was supposed to be intoxicated, so that little notice was taken of him, until a medical man, a passenger by one of the evening trains, pronounced him to be dying, and advised the police to be sent for. He was then removed to a public-house, where I attended him. I found him perfectly insensible; the pupils were contracted; the pulse was scarcely to be felt; the extremities, and indeed his whole body, were very cold. Friction, hot bricks, hot bottles, hot blankets, etc., were applied; but as the police strongly suspected him to be drugged, I considered it my duty (notwithstanding the time that had elapsed) to empty the stomach. I therefore passed the œsophageal tube into the stomach, having the syringe attached, and injected nearly a pint of warm water, when the man vomited, the contents of the stomach passing *by the side* of the tube. I then injected about another pint, and removed the syringe, the fluid then flowed through the tube into a basin. This was repeated until the water was returned perfectly clear, when strong coffee was injected, and the tube withdrawn, the patient being the whole time in the horizontal position. He continued perfectly insensible until between seven and eight o'clock the next morning, making altogether a period of sixteen hours. He recovered, and left the place the same day.

British Medical Journal.

SATURDAY, MAY 5TH, 1860.

THE SANITARY CONDITION OF TROOPS AT SEA.

The ruts of routine are so deeply worn in English official life, that nothing less than a catastrophe which attracts the national attention is capable of lifting our departments out of them. Ever since we have possessed colonies, and have had occasion to provide for their military defence, we have been content with a transport system, every detail of which has been based upon hap-hazard. For a very long time this system has gone on, at the expense of the lives of our soldiers, but without such an overwhelming scandal as should bring the whole matter before the bar of public opinion. The catastrophe has, however, at length come. The frightful scenes on board the *Great Tasmania* we have already alluded to; but since that scandal the public have been startled by the statement that, in five transports lately chartered by the Government to take out the wives and children of soldiers already in India, there has been a loss of no less than *two hundred and seventy children's lives!* We may, therefore, say that the grand crash has come at last; and that, as far as the sanitary condition of troops at sea is concerned, we have everything to reform.

As long as we sent mere driblets of troops to reinforce the small European army formerly kept in India, the necessity of this reform was not so apparent; but now everything in this respect is changed. It is settled that we are to hold India with 80,000 British troops; but these troops, instead of being enlisted for life, as was the case with the majority of them in the time of the East India Company, are now enlisted for ten years service only. It therefore follows that every year, on an average, upwards of ten thousand soldiers will be voyaging outward and homeward, a passage of sixteen thousand miles, to say nothing of the sick who may be passing to the new sanitary station at the Cape of Good Hope. It is evident that we can no longer depend upon the merchant marine for the conduct of a service of this magnitude. At the present moment, it is the custom to take up any decent ship which may happen to be in port for the transport of troops; and this is done with such haste and precipitancy, that she is often chartered for the voyage before she has even discharged her cargo, and only a few days are allowed from this time to the time in which she is expected to be ready for sea. This is the grand secret of the many mishaps which occur to troop-ships, and of the dreadful mortality which, as a rule, obtains on board of them. It is certainly extraordinary that the Commission which inquired into the sanitary condition of the army in 1857 should have ignored altogether the very important subject of the sea-transport of troops. The subject, however, can no longer be tabooed, if every year upwards of ten thousand soldiers are to pass, on an average, four months on the ocean. We must put their *sea-barracks* in the best possible sanitary condition.

A pamphlet by Mr. Charles Kirwan, assistant-surgeon to H.M. 13th Light Infantry, who has made no fewer than nine long voyages in charge of troops, comes opportunely to hand to mark out the course we ought to pursue in organising a new transport service. Mr. Kirwan is of opinion that sea barracks ought to be constructed with as much forethought, and as especially for the purpose they have to serve as land barracks; and he very justly condemns the system of taking up any ship that may happen to be in port.

We should (he observes) organise a fleet of sea barracks, in which our troops should enjoy and maintain a state of comfort, health, and efficiency, inferior only by a few degrees to what they could enjoy in garrison on shore. The *Himalaya* is, it is true, a magnificent vessel, in which a large number of troops can be transported with comparative comfort, but even she is far from being a model. Vessels capable of accommodating a thousand troops, with their due complement of women and children, with their 'tween decks fitted with large ports, like those of the fighting decks of men of war, would be (he thinks) the best suited for the purpose. But the most careful consideration of every detail would be mainly in their construction. The cooking houses, the *latrines*, the slaughtering places, should be so arranged that no offensive smells could with ordinary care hang about the ship. The necessity of perfect cleanliness on board a vessel in the tropics must be obvious to any medical man, but especially to those who may have happened to have had charge of troops at sea. An epidemic once established, the difficulty of ejecting it from the narrow limits of its prison house is only too well known. Therefore the medical officer in charge should be entrusted with

very large powers in all sanitary matters. At present he is but too often treated by the captain merely in his curative capacity; he is told to attend to his hospital, if any matter of mere cleanliness arises between him and the commander of the ship; but it is clear that it is in his capacity as a health-officer that the surgeon of the ship is so valuable. Hence he should have the power, by printed orders, of directing any cleansing of the decks he may think necessary for the preservation of the health of the troops; the awning should also be under his command, and every matter pertaining to the sustentation of a high sanitary condition.

In order to prevent the mandates of the medical officer in charge from being overruled by the commanding officer on light or insufficient grounds, it is proposed by Mr. Kirwan—

"That every troop-ship shall have a 'Sanitary Recommendation Book', in which an officer in medical charge shall write his opinion on every thing likely to act prejudicially on the health of the troops, to be laid before the commanding officer, who will append his approval and publish the recommendation, or note in the same book his objections thereto. If the recommendation objected to be of an important character, copies of such memoranda may be forwarded to the Director-General of the Army Medical Department, and to the Adjutant-General."

At the present moment, the very discreditable manner in which our sea transport service is conducted, may be traced to the fact that no fewer than three departments are connected with the dispatch of the troops by sea—the Admiralty, the Quartermaster-General, with the Commandant at the port of embarkation, and the Council for India—three powers, often pulling different ways! It is obvious that a new service must be established, which shall busy itself solely with the duties of embarking, transporting, and disembarking troops; and at the head of this service should be the Director-General of the Army Medical Department. At the present moment, if anything goes wrong, the blame is shifted from officer to officer, until at last it is proved to the satisfaction of officials that no one is to blame at all; but this would be no longer the case if an official of high position were made responsible. As this question is one of great importance, we shall return to it at the earliest opportunity.

THE WEEK.

At page 349, is published a correspondence which has taken place between Mr. Webber, a member of the Association, and the Council of the Royal College of Surgeons. The correspondence is a *resumé* of certain events which occurred between the year 1854, when the Fellowship of the College was refused to Mr. Webber, and the commencement of the present year. We understand further from that gentleman, that he has prepared a summary of the facts relating to the matter at issue between him and the Council, which (it being too voluminous for the pages of this Journal) he intends to print in another form, and lay before the profession and the public. In the mean time, some extracts will be given in an early number of the Journal, sufficient to explain his present position, and to give the Associates and our other readers a tolerably clear idea of the behaviour of the College towards him. *Primâ facie*, it appears that Mr. Webber has been treated with much injustice and contumely; and it will be for the Council of the College to justify their proceedings, if possible.

About a week ago, we received two numbers of the Guernsey *Comet;* from one of which we learned that John Ozanne, M.D., had received an appointment on the Medical Staff of the Militia of the island from his Excellency the Lieutenant-Governor, while the other stated that, in consequence of a recent appointment, the medical officers of the said force had resigned their posts. What precise connection there was between these two events, or what reason the Guernsey Militia Medical Staff had for acting in so summary a way, we could not at the time clearly divine. We have subsequently learned, however, that Dr. Ozanne is a devotee of the sublime doctrine of homœopathy. No wonder, then, that the medical men of the Guernsey Militia should think that two great lights—the light of medicine and the light of homœopathy—were more than could be borne by the defenders of the island; or that, feeling the light of their new colleague to be much the greater one, they should modestly retire from the vain contest! Joking aside, however, the *esprit de corps* and high professional feeling which our brethren in the Channel have exhibited on this occasion, do them the greatest honour; and their conduct will, we are sure, meet with universal approbation in the medical profession. This is not the first time, indeed, that the medical men of Guernsey have shewn their determined hostility to the homœopathic delusion. When, nine or ten years ago, this Association and other bodies condemned the infinitesimal doctrine, the Guernsey practitioners were among the first to approve heartily of what had been done. They have, on both occasions, and especially in the recent one, set an example which many of greater apparent note in the world would often do well to follow.

The numbers of the *Indian Lancet* for March, which have just reached us, contain an account of a somewhat curious line of conduct adopted towards a medical officer of the Indian army. The subject of the treatment has been Dr. Beetson, superintending surgeon of the Trichinopoly district in the Madras presidency. In the early part of 1858, Dr. Macpherson, recently appointed Inspector of Hospitals, in a demi-official letter to the Director-General of the Medical Department, charging Dr. Beetson with being "the butt of Trichinopoly," with "living in open concubinage with two sisters," with "throwing his house open to all the young fellows, and drinking with them till dawn of day," with "singing a nonsensical song over and over again," with conducting his medical duties in an extremely puerile manner, etc. Dr. Macpherson further expressed his opinion that Dr. Beetson was "in his dotage," and that he was "altogether a disgrace," and tending to "bring the profession to low repute." In consequence of this, Dr. Beetson was called on to tender his resignation; he, however, demanded a full and particular investigation, the result of which was, as we learn, that the charges made by Dr. Macpherson were negatived by the evidence of several officers of the army and other witnesses, while the testimony in support of them was very slight. The effect was to exonerate Dr. Beetson from the charges against his moral character, but at the same time to deprive him of his office as superintending surgeon. This is certainly very unjust; for it appears also, that his professional capability was inquired into, and, instead of being found deficient, was attested to be competent by several respectable witnesses. The course

of proceeding followed in this case is scarcely intelligible to us; so far as we can see, Dr. Beetson has been most unjustifiably persecuted. As might be expected, the periodical from which we have gathered the narrative is highly indignant at the proceeding.

Association Intelligence.

LETTERS AND COMMUNICATIONS.

Letters or communications for the Journal should be addressed to Dr. Wynter, Coleherne Court, Old Brompton, S.W.

Letters regarding the business department of the Journal, and corrected proofs, should be sent to 37, Great Queen Street, Lincoln's Inn Fields, W.C.

SUBSCRIPTION LIST.

The next list of subscriptions paid for the present year will be published on the 12th of May. It is particularly requested that the General Secretary may be put in possession of the names without delay.

Philip H. Williams, M.D., *General Secretary.*

Worcester, May 3rd, 1860.

THE SHROPSHIRE ETHICAL BRANCH.

SUBSCRIPTIONS.

The Council of the Shropshire Ethical Branch beg to remind the members that their subscriptions to the British Medical Association and Medical Benevolent Fund for the current year, became due on the 1st of January, a post-office order for which they will feel obliged by being remitted to the Honorary Secretary (Dr. Styrap, Shrewsbury); punctuality in payment being most essential to the successful progress of the Parent Society.

ROCHESTER, MAIDSTONE, GRAVESEND, AND DARTFORD DISTRICT MEETINGS, IN CONNEXION WITH THE SOUTH-EASTERN BRANCH.

POSTPONEMENT OF MEETING.

In consequence of the annual meeting of the South-Eastern Branch having been appointed to be held at Maidstone early in June, and the annual meeting of the Kent Benevolent Medical Society at the same place early in July, together with the impracticability of obtaining adequate accommodation at Dartford until after the 2nd of May, the district meeting which should have been held at Dartford on the 27th of April has been postponed.

James Dulvey, *Honorary Secretary.*

Brompton, Chatham, April 23rd, 1860.

BATH AND BRISTOL BRANCH: ORDINARY MEETING.

The ordinary meeting of this Branch was held at the White Lion, Bristol, on April 26th, 1860; J. G. Davey, M.D., of Northwoods, President, in the Chair. There were also present:—C. S. Barter, Esq. (Bath); J. Beddoe, M.D. (Clifton); W. M. Clarke, Esq. (Clifton); E. Cockey, Esq. (Frome); G. C. Corbould, Esq. (Bristol); R. S. Fowler, Esq. (Bath); E. L. Fox, M.D. (Bristol); H. E. Fripp, M.D. (Clifton); R. F. George, Esq. (Bath); Thos. Green, Esq. (Bristol); J. Godfrey, Esq. (Bristol); W. B. Herapath, M.D. (Bristol); W. Hutchins, Esq. (Keynsham); H. Marshall, M.D. (Clifton); F. Mason, Esq. (Bath); J. St. J. G. Parsons, L.R.C.P.Ed. (Bristol); J. K. Spender, Esq. (Bath); J. G. Swayne, M.D. (Clifton); S. H. Swayne, Esq. (Bristol); G. Terry, Esq. (Wells); M. Willett, Esq. (Bristol). There were also present as visitors:—Drs. Hobson and Daubeney, of Clifton; Dr. A. Inglis, of Worcester; Dr. Harris, of Madras; and Messrs. Davey, Faull, and E. A. Howsin.

The minutes of the last meeting were read and confirmed.

New Members. H. Marshall, M.D., of Clifton, Jonathan Wybrants, M.B., and Samuel Craddock, Esq., of Shepton Mallet, were unanimously elected members of the Branch.

Papers. The following papers were read:—

1. The Hypodermic Action of Morphia. By J. K. Spender, Esq.

2. Case of Uterine Tumour: Spontaneous Expulsion of Tumour during the third and fourth weeks of delivery. By J. G. Swayne, M.D. [This paper has been received for publication.]

3. Ovarian Abscess. By W. M. Clarke, Esq. Mr. Clarke also exhibited numerous specimens from the museum. [This paper has been received for publication.]

4. Case in which a large Abscess emptied itself by the Bladder: with Recovery. By F. Mason, Esq.

5. Diseased Suprarenal Capsules without bronzed Skin. By E. Crossman, Esq. [This paper has been received for publication.]

6. Dr. Hobson gave some interesting accounts of Feigned Diseases among the Chinese.

Reports of Societies.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, April 24th, 1860.

F. C. Skey, Esq., F.R.S., President, in the Chair.

ON THE TREATMENT OF AXILLARY ANEURISM.

BY JAMES SYME, ESQ.

The object of this communication was to suggest that in general, if not always, the old operation would be preferable to the method of Hunter for the treatment of axillary aneurism. With the view of supporting what might appear rather startling doctrine, the author combated the generally received opinion, that the portion of an artery comprehended by an aneurism is not competent for the process of obliteration by ligature, and maintained that the mere circumstance of isolation from neighbouring connexions while the vessel still retained its usual relation to the sheath should not render it unfit for the process in question. In favour of this position, he quoted the successful result of ten operations performed by him in the old way for the remedy of traumatic aneurism at the bend of the arm, and also the remedy, by the same means, of a carotid aneurism too low in the neck for ligature without opening the sac, which, if allowed to proceed, would have proved fatal, not only to the patient, but to his assailant who inflicted the wound, and who would certainly have been hanged in the event of its proving fatal. The author further related two cases of axillary aneurism, not admitting of relief from ligature of the subclavian, in which amputation at the shoulder-joint was performed with success. But the principal ground of his suggestion was a case of axillary aneurism which had lately come under treatment in the Royal Infirmary of Edinburgh. The tumour was very large, distending the muscles of the axilla, and projecting above the clavicle. It was rapidly increasing, and already showed signs of impending gangrene; while the patient's general condition, as manifested by a pulse of 130, and wandering ideas, was no less alarming. Ligature of the subclavian being quite out of the question, before proceeding to amputation at the shoulder-joint it was deemed proper to ascertain the state of matters in the axilla. But as this could not be done without the risk, or rather certainty, of a fatal hæmorrhage, unless the artery could be commanded in the first instance, an incision was made along the posterior edge of the sterno-mastoid, through the platysma myoides and fascia, so as to allow the finger of an assistant to reach the vessel where it issues from under the edge of the scalenus anticus and lies upon the first rib. The cavity was then opened, nearly seven pounds of coagulated blood removed, and both ends of the artery tied, as it was found to have been torn across. The patient made a good recovery, and was dismissed six weeks after the operation, able to resume his employment. The old operation, having thus succeeded in a case so formidable and unpromising, seemed worthy of adoption, not merely in cases unsuitable for ligature of the subclavian, but as preferable to the Hunterian method, on account of its greater facility, safety, and certainty. The author, having upon two occasions tied the subclavian artery for aneurism with perfect success, had no

prejudice against this operation, but was forced to the conclusion just expressed by his sincere conviction of what was due to expediency. He also begged to suggest that the means employed for effectually commanding the artery perhaps admitted of more extensive application. It had been used by the author thirty years ago in his first operation for the removal of the superior maxillary bone, in which excessive hæmorrhage had been erroneously anticipated, by making an incision between the ramus of the jaw and mastoid process, so as to admit a finger to compress the internal maxillary artery on the neck of the condyle. The author remarked that in this case the superior maxillary bone was removed for the first time in Great Britain, and that the operation is the earliest to be found in the records of surgery.

MEDICAL SOCIETY OF LONDON.

Monday, April 23rd, 1860.

A. B. Garrod, M.D., F.R.S., President, in the Chair.

ON INTRAVASATION, EMBOLIA, AND CYTOSTASIS, AS CAUSES OF CEREBRAL AND OTHER PARALYSIS.

BY J. L. W. THUDICHUM, M.D.

[This paper is published at p. 336.]

Dr. Chowne asked whether Dr. Thudichum had used means for ascertaining the actual pressure on the veins in the case in which he had blown in air.

Dr. Thudichum had thrown air into the jugular vein of a horse from his own lungs, having first ascertained their capacity. He then closed the tube. The breathing of the horse became anxious. In a quarter of an hour, he attempted to blow in more air; but, in doing this, instead of air passing in, blood rushed out up the tube into his mouth.

Mr. Lee asked whether the lower part of the vein was not collapsed in this case. Where was the obstruction?

Dr. Thudichum: In the lungs; these were filled with air, which would not pass on.

Mr. Lee believed that the air coagulated the blood in the vessels of the lungs. Several of the cases related by Dr. Thudichum were of great interest, especially where the abscess opened into a vein. What was the cause of the embolia in these cases? He (Mr. Lee) believed that it was the introduction of pus into the blood; *i.e.*, pus from acute abscess, for pus from chronic abscess may be introduced with comparative impunity. As to the non-rupture of the vessels of the brain, this is accounted for by the circumstance that the coagulation first takes place in the arteries, the stronger vessels. Whenever the blood is fairly obstructed in an artery from coagulation, paralysis occurs; this he believed to be a general rule.

Dr. Leared observed that Dr. Thudichum had laid much stress on adhesion of the blood-corpuscles to each other. He could understand obstruction by detached atheroma or by pus; but he would like to know what meaning was attached by Dr. Thudichum to the process of adhesion.

Mr. Hulke did not think that air coagulated the blood, as supposed by Mr. Lee. In injecting the dead subject, the presence of air in the syringe causes great difficulty; it will not pass through the capillaries.

Dr. Chowne had seen horses killed by blowing air into the veins, and had observed the remarkable obstruction to the passage of the air through the lungs. The right ventricle is full of frothy blood. The vessels were obstructed by air distended by the heat of the body.

Mr. Lee did not mean that air blown into a vein will always coagulate the blood; but it does so sometimes.

Dr. Camps thought some of Dr. Thudichum's cases and deductions open to criticism. With regard to the second case, might not other phenomena be assigned as the cause of paralysis? The same question might be applied to several of the other cases. For instance, kidney-disease might of itself produce paralysis and coma.

Dr. Chowne questioned the connection of arterial obstruction with paralysis. Paralysis does not ordinarily follow ligature of vessels.

Dr. Salter thought Dr. Thudichum's paper interesting, as introducing to notice all the conditions which may arise from the introduction of substances into the arterial and venous circulations. As to the condition of the lung in the case where Dr. Thudichum found hard specks over the organ, it was quite possible that these were a material which is often found scattered through the lung, and which is ordinarily called carbonaceous. He had not examined this chemically but under the microscope he had found it to be perfectly amorphous. The idea that the deposit is carbonaceous was favoured by the facts that the lung-blackening is never seen in very young persons: and that it increases with age; and that it is generally found most in persons who breathe much carbon, especially those who are liable to breathing large quantities of smoke. What reason had Dr. Thudichum for believing that the deposit in his case was decomposed blood? As to the cause of death by the introduction of air into the veins, he believed the former notion was that death was caused by the cessation of the action of the heart on account of the presence of air in the organ. But the remark of Dr. Thudichum as to the great increase of pressure, would lead one to suppose some great obstruction anterior to this point; and he was inclined to agree with Dr. Thudichum that it was in the lungs. Undoubtedly, the air mixed with blood is not driven through a capillary tube so easily as blood itself. With regard to the results of the infarction of vessels with embolia, he had seen a case of which Dr. Thudichum's view no doubt gave the correct explanation. He had been asked to examine an old lady, to see whether she was fit for an operation for cataract. There seemed to be nothing particular the matter; but one morning she complained of intense pain in her left leg. Dr. Salter found the lower part of the limb quite cold, the foot livid, and destitute of ordinary sensibility. The popliteal artery had ceased to beat; and no pulsation could be felt in the femoral artery more than half an inch below Poupart's ligament. She went on in this state several days: and then she began to recover, and got perfectly well. She was then operated on for cataract, and did well. There must have been an impaction of some detached coagulum in the femoral artery. There is no difficulty in explaining the paralysis from obstruction, for the function of any organ or tissue is suspended when its supply of blood is cut off.

The President, several years ago, had made analysis of the black matter in the lungs. It appeared to be pure carbon; it resisted the strongest acids and alkalies, and was entirely dissipated into carbonic acid by heat.

Mr. Lee (in answer to a question from Dr. Chowne) would say that the obstruction by ligature or by vessel was much the same.

Dr. Thudichum, in reply, said the subject which he had brought forward was one of great practical importance in leading to a correct diagnosis. When called to a case of paralysis, one might, but for the knowledge of such facts as he had detailed, often fail in the attempt to make a correct diagnosis; it might even be impossible to make any diagnosis whatever. He had wished to bring forward the secondary effect of obstruction of the vessels on the nervous system. In the cases of introduction of air into the veins, there is a remarkable difficulty of respiration. The entrance of mere pus into the blood is much less likely to cause symptoms of embolia than the passage of detritus.

Monday, April 30th, 1860.

A. B. Garrod, M.D., F.R.S., President, in the Chair.

Eczematous Eruption following the Administration of Chloroform. Mr. J. F. Clarke asked whether any Fellow had observed any effects on the skin produced by the inhalation of chloroform. A lady had been under his care, who had required to take chloroform several times. This was at last followed by an eruption resembling scarlet fever, but which proved to be eczema. It covered the body, and desquamation was most copious.

Dr. Salter had seen some cases in which erythema had followed the administration of iron and bark. He had never seen anything of the kind from chloroform.

ON INJECTIONS OF TRISNITRATE OF BISMUTH IN GONORRHŒA.

BY VICTOR DE MÉRIC, ESQ.

Mr. De Méric observed, that numerous remedies were already employed in the treatment of gonorrhœa; but, as bismuth injections had been much used of late on the continent, he had thought it best to try them on a large scale. As to the pathology of the disease, he held that it was non-syphilitic when uncomplicated with chancre, but that the disease contains a special poison. By the side of this special disease, we find discharges from the male urethra, arising from mechanical irritation, or from the contact of leucorrhœal matter. The broad distinctive characters of the specific and non-specific forms are, that in true gonorrhœa inflammation is present, followed by a decidedly purulent discharge; while the simple

excessive secretion, arising from irritation, etc., is never accompanied by phlogistic symptoms, and mucus only is discharged. It is often, however, very difficult in practice to make the diagnosis. It is often observed that some patients recover very soon, while others require a long course of treatment. In the treatment of the simple secretion, arising probably from leucorrhœal discharge, he had for some years used an injection recommended by M. Ricord, of acetate of lead and sulphate of zinc, which, when mixed, underwent double decomposition. He was therefore prepared to use an insoluble salt, when he began to try the trisnitrate of bismuth. He had employed this remedy on the out-patients at the Royal Free Hospital. There were difficulties in the investigation, from the impossibility of making correct observations on out-patients. From various reasons, experiments were more easily performed on such cases in France than in England. Injections of trisnitrate of bismuth had been highly recommended not only in males, but in uterine and vaginal discharges in females: he had, however, hitherto confined his attention to the treatment of gonorrhœa in males. He treated the acute period of gonorrhœa by antiphlogistics and rest. He had not combined internal remedies with the injections; but had prohibited spirits, malt liquor, and sexual intercourse. He had ordered at first half a drachm to the ounce of water, but had soon reached a drachm to the ounce, which proportion he had mostly used. This injection had been highly recommended in Paris by a French surgeon. It was principally in the chronic form that the remedy had been used. The French author had related forty-three cases of cure by the injection, without the use of internal remedies; the period of treatment being from three to twenty-one days. He had not, however, stated whether there were relapses. In France, the glans was powdered with the trisnitrate in balanitis, and wrapped in cotton wool; but Mr. De Méric would not use this envelope, for fear of paraphimosis. Mr. De Méric's patients were ordered to use the injection four times a day; but it might be supposed that the injections were made three times a day. The patient sits on a chair after passing urine; and, having injected the whole contents of the syringe, holds the meatus for a time, to prevent the exit of fluid.

The experiments were begun in January last, and in sixteen weeks had been tried on one hundred and forty patients. A good many were still under treatment, some had attended irregularly, and many had not reappeared; so that there remained fifty-two cases in which the result was accurately known. These were divided by the author into three classes: 1. Those who remained well after the end of a week; 2. Those who had not reappeared at the hospital after being declared much better; 3. Patients in whom the remedy failed completely.

In the first class were thirty-six cases, in which the shortest period occupied in a cure was seven days, the longest thirty-seven days, the average period being twenty-two days. Thirteen of these were first attacks. In the second class, there were five cases, of whom four were much improved in seven days, and one in thirty-three days. In the third class, that of failure, there were eleven patients, all of whom recovered by the use of cubebs and copaiba. By adding the first and second classes together, there appeared to be forty-one successful cases to eleven failures.

The author was aware that his figures were not conclusive, and that eighty-eight registered patients remained unaccounted for. He thought, however, that the statistics proved that bismuth, though it was not likely to supersede other remedies, was nevertheless useful. The cures were all pretty complete. He always ordered the patients supposed to be cured to leave off injection, and to live freely, so as to fairly test the effect of the remedy. In three or four cases only, a relapse took place. In the fifty-two cases, two only had swelled testicle; but one of these had double orchitis. One had a chancre at the end of the penis, and a few others had various complications.

In patients using the bismuth injection, a white deposit is observed round the meatus, and the skirt is generally marked. In three or four cases in private practice, he had used half a drachm to the ounce, four times a day. In four cases only, where the drachm to the ounce was used, were there unpleasant symptoms. In two, there was hæmorrhage from the urethra; but in one only was it necessary to leave off the bismuth; one patient had pain in the bladder, and another was troubled with frequent micturition.

There is but little probability of the bismuth forming the nucleus of a stone in the bladder. Injections, as generally practised, do not pass further than the membranous portion of the urethra; and, if any reach the bladder, it would probably be washed out by the urine. It may possibly produce hæmorrhage by the sharp edges of its particles wounding the mucous membrane of the urethra. Mr. De Méric did not believe the use of the bismuth injection likely to produce stricture. He believed from his own experience that caustic injections are not so often the cause of stricture as is supposed. He had used caustic injections in gonorrhœa without observing stricture; and the introduction of caustic solutions into the larynx does not produce stricture of this part.

How does the bismuth act when it is injected, suspended in water? It is supposed that a portion of the bismuth remains in the urethra, and, resting in the follicles or lacunæ, exerts its action on them. By micturition, the bismuth is washed out; the first part of the stream of urine being of a white colour.

[Mr. De Méric handed round some instruments designed for applying caustic to the urethra.]

Mr. J. F. Clarke said that, in arriving at a just conclusion as to the value of the remedy, the statistical information should be more complete, especially with regard to the previous treatment. He believed there was a tendency in the gonorrhœal discharge to leave off—not, however, that the disease would absolutely get well of itself without medicine. As far as he could gather from the paper, the remedy was very valuable in the treatment of long standing cases of chronic gonorrhœa.

Dr. Hardinge asked what was the *modus operandi* of the bismuth.

Mr. H. Smith, in reference to the instruments handed round for application of caustic to the urethra, referred to one of them which had been introduced before the Society ten years ago by himself, and which, in investigating the treatment of gonorrhœa, he had devised as a means of applying a solution of nitrate of silver to the urethra. He thought that, as the author had mentioned M. Ricord as the inventor of one of the instruments, he might have made an analogous statement with regard to the other.

Mr. Bryant said that, if Mr. De Méric advocated the use of bismuth in *ordinary* cases of gonorrhœa, he did not think that the remedy was superior to others, such as salines, copaiba, etc., without injections; nor was it superior to injection of tannin. But, in very chronic and obstinate cases, he would be much inclined to try the bismuth injection. He asked how it was suspended in water. Was any gum-water added? He would ask Mr. Smith what success he had had with the instrument to which he had referred.

Mr. H. Smith did not use caustic applications to the urethra nearly so frequently as he formerly did.

Mr. Miller thought injections in gonorrhœa often very injurious. There was, however, apparently no harm, at least, produced by trisnitrate of bismuth.

Mr. Bryant had been in the habit of inquiring into the statistics of stricture, with regard to injections. Roughly speaking, and without having collated his facts, out of about a thousand cases, in two-thirds there had been gonorrhœa, but not in more than a third of these had injections been used.

The President said that it would at first sight seem probable that bismuth would prove valuable in certain states of the urethra, from its effects on other mucous membranes. We know nothing of the action of trisnitrate of bismuth, beyond the fact that it acts locally. He had tried the soluble salts, as the citrate, etc., in pyrosis; but without effect. He would ask if any Fellow of the Society had any acquaintance with the use of carbonate of bismuth. Was it better than the trisnitrate?

Dr. Hardinge had found the carbonate of bismuth more efficacious and more agreeable to the patient than the trisnitrate; and he of late had used it, in the same way as the trisnitrate, to the exclusion of the latter remedy.

Mr. De Méric, in reply, acknowledged the necessity for further statistical information. As to the action of bismuth, he believed it to be only mechanical; probably it was astringent or drying. It appeared to coat the urethra, so that, after its use, patients passed their urine more easily. He would not use the injection of bismuth in common cases, but only in those where other remedies seemed likely to fail. The bismuth was used with water alone; he had not added gum. In reply to Mr. Smith, he said that he was aware that, several years ago, there was a controversy between Mr. Smith and another surgeon as to the priority of invention. He had therefore abstained from mentioning any name, but was quite ready to give Mr. Smith the credit of designing the instrument; which he (Mr. De Méric) had described as being in the hands of every surgeon.

Editor's Letter Box.

SUSPECTED POISONING BY OXALIC ACID.

Sir,—Having some years ago been engaged in investigating a case, in which a servant attempted to poison her mistress with oxalic acid, and having made many experiments in relation to the properties of this acid, I venture to make the following remarks upon the case of suspected poisoning published in the Journal of February 11th, by Dr. Skinner, of Liverpool, as in my opinion the facts by no means bear the inferences he has drawn from them.

The grounds of his belief that his patient was poisoned are, in brief: 1, a sudden attack of peritonitis in a healthy female; 2, a relapse without evident cause; 3, the death of the leeches applied on each occasion, though others used previously had survived their application; 4, a constant and unaccountable nausea and oppression; 5, a sudden burning pain in the stomach, with vomiting and collapse; 6, spasmodic twitchings of the neck and limbs the following day.

The above are the data for fixing on oxalic acid as the poison employed; and a heartless indifference to the condition of her mistress (a single lady, with no near relatives, of whose service she was tired), is the ground for fixing on the servant a charge of attempt to murder by slow poisoning.

Let us examine these points in detail.

1. A sudden attack of peritonitis, or any other inflammatory complaint, may seize a person in sound health, without any suspicion of poisoning by oxalic acid, which would be more likely to produce gastritis than peritonitis. 2. A relapse is not an extraordinary circumstance in any disease, nor is it extraordinary (4) that such an attack should be followed by nausea and oppression, which should be brought to a climax in an attack of (5) burning pain and vomiting, lasting many hours; and terminating in (6) spasmodic twitches, followed by a return to health the next day. The third point, the death of the leeches, viewed in connexion with the vaginal discharge following the attack of vomiting, appears to me to afford a clue to the whole case, without referring to oxalic acid or any other irritant poison. It has so frequently occurred to myself to find that leeches die when applied to patients suffering from uterine and hysterical complaints, that I am convinced that a morbid change exists in the blood of many women at such times, though I do not profess to know its nature, and there is no more common symptom of such affections than abdominal pains, which almost exactly simulate true peritonitis. Again, nausea and oppression, followed by violent bilious vomiting, are also by no means unusual concomitants of such attacks, which are often brought to a close by the discharge of a morbid menstrual secretion, and are not infrequently followed by spasmodic affections of various kinds. Such is my view of the nature and cause of this case, viz., that it was a form of hysteria.

But, supposing I have taken an erroneous view, there is little or no ground for supposing it to have been a case of irritant poison; still less for selecting oxalic acid, for the following reasons:—1. The taste of oxalic acid is so intensely sour, that it could not be disguised in any attempt at slow poisoning; and yet all the proof alleged is, that some gruel returned from the stomach sourer than it went down; a proof, if anything, that the acidity was in the stomach, and not in the gruel. 2. The patient is said to have completely recovered from the deadly collapse, pain of stomach, etc., the next day, whereas it is a well known fact that the recovery from the effects of irritant poisons is very slow and imperfect. 3. The spasmodic twitchings, upon which Dr. Skinner lays the greatest stress, as proofs of oxalic acid poisoning, only lasted a day, whereas, in the cases he cites in support of his argument, they continued in one for a month, and in the other for fourteen days. 4. The smallness or dilution of the dose supposed to have been given is inconsistent with an attack of pain and vomiting, with collapse of twenty hours duration.

In short, sir, I think that you and the profession generally will agree with me that an ill-conditioned spiteful maid is not to be thought a murderess because her mistress has a pain in the abdomen, for which, when leeches are applied, they die; and because she has an attack of sour bilious vomiting, with pain and collapse of twenty hours duration, followed the next day by spasmodic twitchings, and the day after by complete recovery; particularly when, by a careful analysis, not an atom of any poisonous substance could be detected in six ounces of the matter vomited. I am, etc., Justus.

April 1860.

THE PROPOSED NEW LICENTIATES OF THE COLLEGE OF PHYSICIANS.

Sir,—In reference to the intention of the Royal College of Physicians of London to institute a new order of licentiates, who may supply medicines to their patients, the following important points deserve consideration.

1. Whether these licentiates are to be *bonâ fide* physicians or only apothecaries under another qualification.

2. If not physicians, what are they? They certainly cannot call themselves either surgeons or apothecaries. Under what title or name then are they to practise?

3. If physicians, are they to describe themselves as "Dr. Jones" or as "Mr. Jones, Physician." The former title, M.D.'s say physicians have no right to assume; and to the latter mode it is objected, that it would tend to lower the name, and present high status of the physician.

4. Whether medical practitioners should be admitted without examination. It certainly would be strange that what has been so strongly condemned (even by the Medical Council) and abandoned in the Edinburgh College should be approved of and thought quite right in the London one. I believe there are very few of the Edinburgh licentiates who do not regret that they did not obtain their diploma after examination.

5. What relation will licentiates of a Royal College of Physicians bear to graduates in medicine? If the Colleges of Physicians give neither title, rank, or name, to their licentiates, will not gentlemen rather go to Aberdeen or St. Andrew's for a degree which will make them at once doctors, esquires, and physicians?

6. Are not physicians, however, as much doctors, as doctors are physicians? If the graduate's license of practising (*exercendi*) medicine constitutes him a physician, does not the physician's license of teaching (*docendi*) medicine constitute him a *doctor?* If not, why not? Does not the title, in each case, flow from the privilege? I am, etc.,

An Edinburgh Physician.

Bristol, April 1860.

LA CRÊCHE, OR INFANT NURSERY.

Letter from F. J. Brown, M.D.

Sir,—I visited, yesterday morning, the *crêche* or infant nursery lately established at Brompton, Chatham. The institution is new to me; and a description of it may prove interesting to those of my professional brethren that have not seen anything similar.

The building, newly erected, is situated near to Trinity Church, and consists of one large room of high pitch, with a kitchen and scullery. The apartments are all ventilated. In the large room there are two cribs capable of containing several infants, and benches suitable for young children. There are a matron and three assistants—the latter being girls of twelve or thirteen years of age, apparently. The assistants teach the alphabet by means of a large pattern; and they sing to the children, and teach them little hymns. The establishment is open from 6.30 A.M. to 6 P.M. in winter, and one hour later in the evening in the summer. It is open only five days in the week, being kept closed on Saturdays and Sundays. There is an average attendance of thirty-nine children. The cost of the establishment is defrayed by subscriptions and donations. A small sum is charged the parents of the children; viz., two-pence a week to those in the army or navy; two-pence a day to civilians.

Food is freely provided:—bread and milk, stews, soup, and rice-milk. A medical practitioner in the neighbourhood attends the children gratuitously, if they are taken ill at the nursery. The children are clean and warmly clad, and of healthy appearance. Several of the younger ones were sleeping in the cribs at the time of my visit, covered by clean blankets, whilst the elder ones were following one of the assistants in learning the alphabet, and others were repeating or singing "Gentle Jesus, meek and mild"—a well known infant's hymn.

Children are admitted to the establishment between the ages of two months and four years. The mothers are permitted to come to the nursery at any time during the day to visit their young ones or to suckle them.

These infant nurseries differ from ordinary schools for young

children in many particulars—such as early age of admission, feeding, sleeping-cribs, assistant nurses, ventilation, and general superintendence.

It is the duty of the rich to provide infant nurseries in all towns, so as to do away with the sad necessity of mothers placing out their babes with ogresses that drug the little ones. Let us assist the poor to remedy this wretched system of child murder. I am, etc.,

Fredk. J. Brown, M.D.

Rochester, Kent, April 28th, 1860.

Parliamentary Intelligence.

HOUSE OF COMMONS.—Thursday, April 26th, 1860.

Military Surgeons. Mr. Deedes asked the Secretary of State for War upon what principle a deduction of 8½*d.* per diem was made from the pay of the surgeon and assistant-surgeon of a cavalry regiment for each horse kept by them for the public service, when the rate of pay of those officers was precisely the same as that of the medical officers of infantry regiments who, under the twenty-first paragraph of the Warrant of the 1st of October, 1858, were expressly exempted from any such deduction, the words of the paragraph referred to being these:—"Shall not in future be subject to any stoppage out of their daily pay for any ration of hay, straw, or oats, supplied for the horse or horses kept by them for the public service."

Mr. S. Herbert said the case was as stated, and the papers on the subject were before the House.

Friday, April 27th.

Office of Coroner. Mr. Cobbett asked the Secretary of State for the Home Department whether it was his intention to propose to Parliament any measure to carry into effect the recommendations of the Select Committee on the Office of Coroner?

Sir G. Lewis said that there was a Bill before the House giving an appeal to the Queen's Bench in disputes between the county magistrates and coroners; and several of the recommendations of the Select Committee he should be disposed to adopt, either by incorporating them in that Bill, or by introducing them into a new Bill. He confessed, however, that he was not disposed to adopt the recommendation that the coroner should be paid by salary.

Medical News.

BIRTHS, MARRIAGES, DEATHS, AND APPOINTMENTS.

** In these lists, an asterisk is prefixed to the names of Members of the Association.*

BIRTHS.

Of sons, the wives of—

*Fletcher, F. D., Esq., Liverpool, on May 2.

Keys, George F., Esq., Surgeon, Warwick Street, on May 1.

Of daughters, the wives of—

Brown, Henry, Esq., Surgeon, Windsor, on April 29.

Crosby, Thomas B., Esq., Surgeon, Finsbury Place, on May 2.

Ellison, James, M.D., Windsor, on April 30.

Everest, Henry, Esq., Surgeon, Westbourne Terrace Road, on April 30.

Grayling, George, M.D., Forest Hill, on April 27.

MARRIAGES.

Brown, John, jun., Esq., Great Yarmouth, to Katherine W., daughter of the late David Mignot, M.D., at Stradishall, on April 26.

Dauney, Alexander, Esq., of the Middle Temple, barrister-at-law, to Emily Ellen, only daughter of John Hastings, M.D., of Albemarle Street, on April 30.

Hitchen, Isaac, Esq., Alsager, Cheshire, to Elizabeth C., eldest daughter of the late *Thomas W. Smith, M.D., Stroud, on April 25.

Hughes, Adolphus J., Esq., Surgeon, to Elizabeth S., eldest daughter of W. J. Elwin, Esq., of Dartford, on April 26.

Marriott, Charles W., Esq., Surgeon, Leamington, to Sarah Ann, eldest daughter of Joseph Taylor, Esq., of St. Clement's Lane, London, at Camberwell, on April 26.

Preston, Lieutenant Barnard H., 2nd European Madras Light Infantry, to Helen A., youngest daughter of Charles Ford, Esq., Surgeon, of the same regiment, at Trichinopoly, on March 13.

Salmon, James, Esq., Deputy Inspector-General of Hospitals and Fleets, to Louisa, daughter of the late Lieutenant Nelson Elliott, R.N., at Alverstoke, on April 28.

Watling, Henry W., Esq., Surgeon, to Emily, eldest daughter of the Rev. Charles H. Watling, B.D., of Tredington, on April 30.

DEATHS.

Blake. On April 28, at Torquay, Mary, wife of C. Paget Blake, M.D.

Hooper. On April 27th, at Cheltenham, aged 3 years and 8 months, Russell Hicks, youngest son of W. H. Hooper, M.D.

Huxley. Lately, at Barming Heath, near Maidstone, Mary, wife of James E. Huxley, M.D., Resident Physician of the Kent County Lunatic Asylum.

Mushet, Geo., M.D., of Essequibo, at St. Kitt's, on March 24.

Pinhey, Robert, Esq., late Surgeon-General of the Bombay Army, at Karsfield, near Exeter, aged 67, on April 28.

Waite. On April 23rd, aged 41, Isabella, wife of George Waite, Esq., Surgeon, of 2, Old Burlington Street.

Walker, Thomas, M.D., late Physician to the Forces, at Inveravon, aged 77, on April 24.

*Willcox, Joseph, Esq., at Warwick, aged 59, on April 12.

Wyman. On April 28th, at Alcester, Mary Charlotte, wife of *George Wyman, Esq.

APPOINTMENTS.

*Garrod, Alfred B., M.D., F.R.S., appointed Examiner in Materia Medica in the University of London.

*Smith, W. Tyler, M.D., appointed Examiner in Midwifery in the University of London.

Watson, Patrick H., M.D., appointed Assistant-Surgeon to the Edinburgh Royal Infirmary.

Royal Medical Benevolent College. The eighth annual festival of this institution took place on Thursday week, at the Freemasons' Tavern, when a very large number of the friends and supporters of the College sat down to an excellent dinner, provided by the establishment in their usual style. The Right Hon. Lord Leigh presided on the occasion, and was supported by Viscount Newark, Sir Charles Locock, the Rev. George Pocock, Colonel Burlton, Dr. Conolly, Mr. Briscoe, M.P., Mr. R. A. Gray, Dr. Hogg, etc. The usual loyal and patriotic toasts having been proposed and responded to, the noble chairman called upon the company to drink "Success to the Royal Medical Benevolent College," and in doing so the noble lord spoke in terms of the highest praise of the members of the medical profession, whose duties to society he described to be of the most valuable and, at the same time, the most onerous and responsible description. Mr. Propert, the treasurer, stated that the receipts of the institution during the past year were as follows:—Annual subscriptions, £3,062: 18: 6; donations, £2,163: 9; legacies, £792: 8; dividends on stock invested for endowment fund, £106: 10; payment from exhibitioners and day scholars for board and education, £4,545: 2, making a total of £10,670: 7: 6. There were now at the College 113 exhibitioners (or boarders, paying the exact cost of their board and education), 19 day scholars, and 36 foundation scholars, who are educated, boarded, and clothed at the expense of the institution, and 4 foundation scholars are to be admitted at the election next month—total, 172. There were also 19 pensioners provided with furnished residence and an allowance of £15 a year each, and one more is to be admitted next month. A statement of the contributions during the evening was then read, the total sum amounting to upwards of £1,000. The Quartet Glee Union agreeably diversified the proceedings, and altogether a most successful anniversary festival favoured this charitable institution.

Epidemiological Society. On Monday, May 7th, 1860, a paper will be read on "The Topography and Prevailing Diseases of the Gold Coast, West Coast of Africa." By Robert Clarke, Esq., late Physician to the Africans on the Gold Coast.

MR. WEBBER AND THE ROYAL COLLEGE OF SURGEONS.

WE have been requested to publish the following correspondence:—

I.—*Mr. Webber to the President and Council of the Royal College of Surgeons.*

"Cadogan Hotel, Sloane Street, Feb. 21, 1860.

"MR. PRESIDENT AND GENTLEMEN,—After more than five years of anxious, painful, and wearing endurance, of the vast and unqualified wrong you readily inflicted upon me, and of your refusal to concede me a fair and just hearing, I have at last succeeded in sifting out the (scandalous) grounds upon which you, by *ex parte* procedure, essayed to deny me the Fellowship of the College; a fiat which, as if to render it in every way as hurtful to me as possible, was within a very few hours afterwards proclaimed to the world in the pages of the *Lancet*. It is not my purpose here to comment upon a course so undignifying and un-English, or upon an act of injustice so deliberate and monstrous, but merely to repeat for the last time (and without prejudice to the steps I may find it necessary to take) my oft-reiterated request, that you will without delay agree to the appointment of a competent and disinterested committee to make full inquiry into the whole matter, with power to report thereon, in order that my grossly and covertly-assailed character may be relieved, as far as can now be done, from the wide-spread aspersions and deeply-rooted prejudices you, without hesitation, signally contributed to attach to it; that my (rightful) claim to the Fellowship (you have lately accorded to me) may be honourably established, and the cowardly calumniators of my good and hard-earned repute, and destroyers of the promising prospects I had secured, may be brought forth to answer at the bar of public justice for the irreparable damage occasioned me and my family. Waiting an early reply. "I am, Mr. President and Gentlemen,

"Your obedient servant,

"WM. WEBBER, F.(elect)R.C.S.

"To the President and Council of the Royal College of Surgeons, Lincoln's Inn Fields.

II.—*The Secretary of the Royal College of Surgeons to W. Webber, Esq.*

"Royal College of Surgeons, London, W.C., 15th day of March, 1860.

"SIR,—Your letter of the 21st ultimo, 'requesting the Council of this College to agree to the appointment of a competent and disinterested committee, to make full inquiry into the matter of your non-election to the Fellowship of the College in 1854, with power to report thereon, in order that your grossly and covertly-assailed character may be relieved, as far as can now be done, from the wide-spread aspersions and deeply-rooted prejudices, in order that your claim to the Fellowship lately accorded to you may be honourably established,' has been laid before the Council. I am desired to acquaint you, in reply, that your application is one which the Council cannot entertain; adding, that the Council were not in any way parties to the publication in the periodical to which you refer. "I am, sir, your most obedient servant,

"Wm. Webber, Esq." "EDM. BELFOUR, *Sec.*

III.—*Mr. Webber to the President and Council of the Royal College of Surgeons.*

"Bognor, March 31st, 1860.

"MR. PRESIDENT AND GENTLEMEN,—Mr. Belfour's letter of the 15th inst., in reply to the one I addressed to you on the 21st ultimo, asking for an early answer, has been forwarded to me.

"In refusing me a request which is considered to be a very reasonable and temperate one by the several fellows and members of the College whom I have consulted upon the subject, I regret that you should have found it expedient to string together half-sentences and garbled extracts from my letter as a means of blinking a charge of gross injustice, of staving off a fair and due inquiry, and of turning away from making an honourable and manly effort to redress, as far as in your power lies, the great and unqualified injury you have done me.

"I am now left no alternative than publishing the correspondence which has passed between us; laying the naked facts before the profession, and asking at the hands of my compeers that justice which you have denied me.

"I am, Mr. President and Gentlemen,

"Your obedient servant, "WM. WEBBER.

"The President and the Members of the Council of the Royal College of Surgeons, London."

IV.—*The Secretary of the College of Surgeons to W. Webber, Esq.*

"Royal College of Surgeons, London, W.C., April 17th, 1860.

"SIR,—I am desired to acquaint you that your letter of the 31st ultimo has been laid before the Council of this College.

"I am, sir, your obedient servant,

"William Webber, Esq." "EDM. BELFOUR, *Sec.*

V.—*Mr. Webber to the Secretary of the Royal College of Surgeons.*

"Bognor, April 18th, 1860.

"SIR,—I have received your letter of yesterday's date, acquainting me 'that my letter of the 31st ultimo', addressed to the President and Members of the Council, 'has', like the former one of the 21st of February, 'been laid before the Council of the College'; with, it would seem, as little chance, although long sat upon, of being hatched off satisfactorily, but which ought to have been the case weeks—nay, months—ago, had they been as prompt to redress a wrong as they were ready to refuse a right.

"After the long deferred reply of the Council, on the 15th of last month, to my letter of the previous month, in which they persist in declining to entertain my frequent and fair request, I did not expect any further communication from them. I therefore, in accordance with the intimation conveyed by me in that letter, have caused the correspondence to be published in one periodical, and I shall send it to others for the same purpose. I have no desire to reciprocate the conduct which the Council have pursued towards me, by the exercise of any harsh or vindictive feeling; but I cannot allow the letter you have been directed to write me to prejudice my position, or to interfere with the course I have been advised to take, by assenting to the further procrastination of a question which, as honourable men, aware of the serious wrong they have committed, they ought to have felt no difficulty in at once meeting (at the onset) in a manner equitable to me, and in a spirit and way creditable to themselves.

"They cannot, I imagine, desire or expect to impose an obligation in conceding a measure which I do not ask as a favour, but claim as a right, and am, moreover, entitled to demand as an act of justice. "I am, etc., "WM. WEBBER.

"To E. Belfour, Esq., Sec. of the Royal College of Surgeons."

PROSECUTION UNDER THE MEDICAL ACT. At the Thames Police Court, on Thursday week, Mr. Talbot, a man about 50 years of age, better known as Dr. Talbot, appeared before Mr. Selfe, to answer summonses taken out under the "Medical Act," which charged him with having, on the 28th of November last, and on divers other days, wilfully and falsely pretended to be, and taken and used the names and titles of physician and doctor of medicine, implying he was registered under the Medical Act, by which he had incurred a penalty of £25. The court was crowded with members of the medical profession and others. Mr. Charles Young, solicitor, of Stepney, conducted the prosecution for Dr. Wm. Giles. Evidence having been adduced to prove the charge, Mr. Selfe said this was a most proper case for prosecution, and fined the defendant £10. At the same court, on Wednesday last, the prosecutor in the case, Dr. Giles, applied to Mr. Yardley, the sitting magistrate, for a warrant against Talbot, for a series of foul libels on him. The warrant was granted.

HEALTH OF LONDON—APRIL 28TH, 1860.

[*From the Registrar-General's Report.*]

	Births.	Deaths.
During week ..... { Boys.. 914 / Girls.. 885 }	1799	1268
Average of corresponding weeks 1849-58	1650	1183

Among the causes of death were—bronchitis, 145; pneumonia, 75; phthisis, 198; small-pox, 20; scarlatina, 33; measles, 37; diphtheria, 9; hooping-cough, 56. The deaths from pulmonary diseases (exclusive of phthisis) were 240, being 55 above the corrected average.

Barometer:

Highest (Sat.) 30·16; lowest (Mon.) 29·571; mean 29·855.

Thermometer:

In sun—highest (Sat.) 104·0 degrees; lowest (Wed.) 45·0 degrees.

In shade—highest (Sat.) 58·3 degrees; lowest (Mon. & Sat.) 31·3 degrees.

Mean—41·2 degrees; difference from mean of 43 yrs.—6·6 degrees.

Range—during week, 27·0 degrees; mean daily, 18·0 degrees.

Mean humidity of air (*saturation* = 100), 83.

Mean direction of wind, N.E.—Rain in inches, 0·45.

TO CORRESPONDENTS.

Communications have been received from:—DR. LIONEL BEALE; DR. F. J. BROWN; MR. JAMES ROUSE; MR. E. CROSSMAN; MR. R. S. FOWLER; DR. J. G. SWAYNE; DR. MCWILLIAM; DR. W. A. GUY; DR. JAMES RUSSELL; DR. THUDICHUM; DR. F. T. BOND; DR. W. T. FOX; MR. W. M. CLARKE; DR. OGIER WARD; DR. STYRAP; MR. JOHN DIX; MR. POSTGATE; DR. J. E. CROOK; SIR C. HASTINGS; MR. H. R. HAMILTON; M. LE DR. MÉRAN; MR. F. D. FLETCHER; DR. J. G. WILSON; DR. W. NEWMAN; and AN EDINBURGH PHYSICIAN.

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NOTICE.—S. ISAACS & SON beg to return their sincere thanks to the Medical Profession for past favours, and beg to inform them that in consequence of the great increase in their business, they have removed to more extensive premises, and hope by adhering to their usual attention and punctuality to receive a continuance of their favours at their new London Warehouses, 24 and 25, Francis Street, Tottenham Court Road. Packages free. Remittance on receipt of Goods Post-office Orders payable to S. ISAACS and SON, at the Post Office, Tottenham Court Road. Bankers—Unity Bank.

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NEW MEDICAL GLASS BOTTLES and PHIALS, at the Islington Glass Bottle Works, Islington Place, Park Road. LONDON WAREHOUSES—19, Bread Street Hill, Upper Thames Street, City, and 2, Upper Copenhagen Street, Barnsbury Road, Islington. E. & H. HARRIS & Co., beg to submit the following prices for quantities of Six Gross, assorted to suit the convenience of the purchaser.

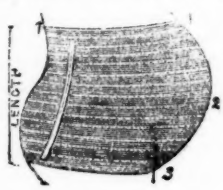
6 and 8 oz., any shape, plain, or graduated	clear	8s. 0d.	per gross.
3 and 4 oz. ditto ditto	blue tinted	7s. 6d.	„
½ oz. White Moulded Phials	of a very superior quality.	4s. 6d.	„
1 oz. ditto		5s. 6d.	„
1½ oz. ditto		6s. 0d.	„
2 oz. ditto		7s. 0d.	„

Immediate attention to country orders. No remittance required until the Goods are received. Packages free. Goods delivered free within seven miles. Post-office Orders made payable to E. & H. HARRIS & Co., at the Chief Office, London. Bankers—Union Bank of London.

N.B. Orders sent to either establishment will meet with prompt attention.

Mr. Bourjeaurd's Spiral Elastic

COMPRESSIONAL APPLIANCES, Registered, to be obtained only at No. 11, DAVIES STREET, BERKELEY SQUARE, W., and at No. 11, RUE DES BEAUX ARTS, PARIS, and through Medical Practitioners.



Mr. BOURJEAURD, in submitting the accompanying Diagrams, presents his best thanks to the Members of the Medical Profession for their unceasing support of his invention of the Spiral Elastic Compressional Principle in Surgical Appliances, and for the continuance of recommendations in all parts of the kingdom. At the same time, he feels it incumbent upon him to remind his friends, that this Invention was registered by him in February 1848, and, immediately after, advertised in the Medical Journals; thus affording an undeniable contradiction to the assertions of the appropriators of his principle, who have never appeared till within the last three and a half years, and yet style themselves Patentees, Registrars, etc., of the Spiral Principle. Mr. B. having served as Surgeon in the French Navy has had great experience in bandaging; he has likewise been established eleven years in London, confining his attention entirely to cases indicating the necessity of compression; and begs leave to state that it is his firm impression that Patients should have the assistance of their Medical Attendant whenever the Spiral Elastic Bandages are applied, or otherwise should be attended by himself, as it must be admitted to be exceedingly hazardous for the suffering to entrust their limbs or cases of Hernia to persons totally ignorant of anatomical or physiological laws.

Mr. B. begs to state that he has very much enlarged his Manufactory, and is prepared to supply these valuable appliances, wholesale and for exportation, 30 per cent. lower than the imitations.

N.B. Near Mivart's Hotel. No. 11, DAVIES STREET, BERKELEY SQUARE, between Brook Street and Grosvenor Street. The Establishment is strictly private; no shop or show of bandages in the window. At home from 10 till 6. Female Attendance for Ladies.